

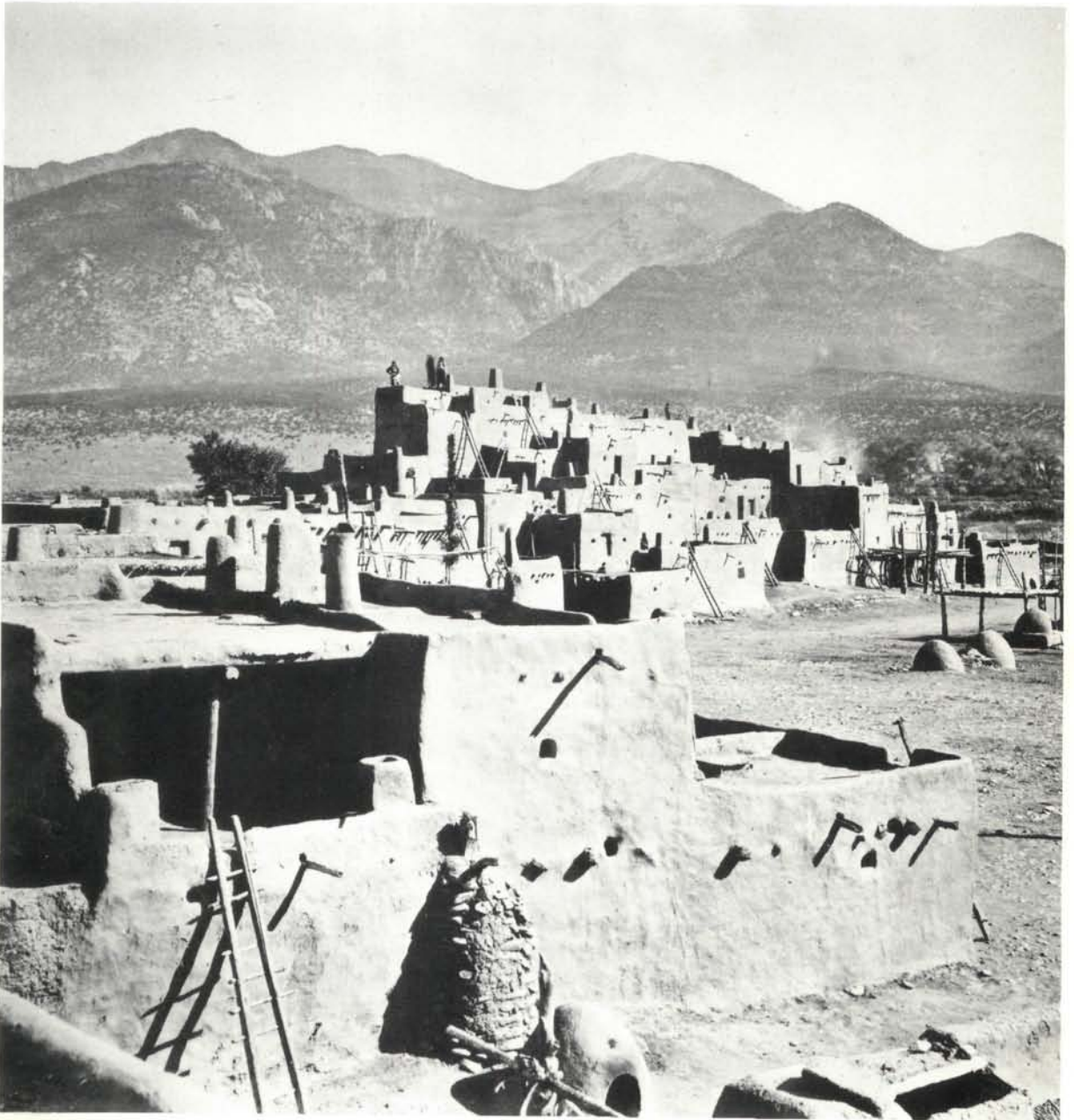
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# The American Historical Review

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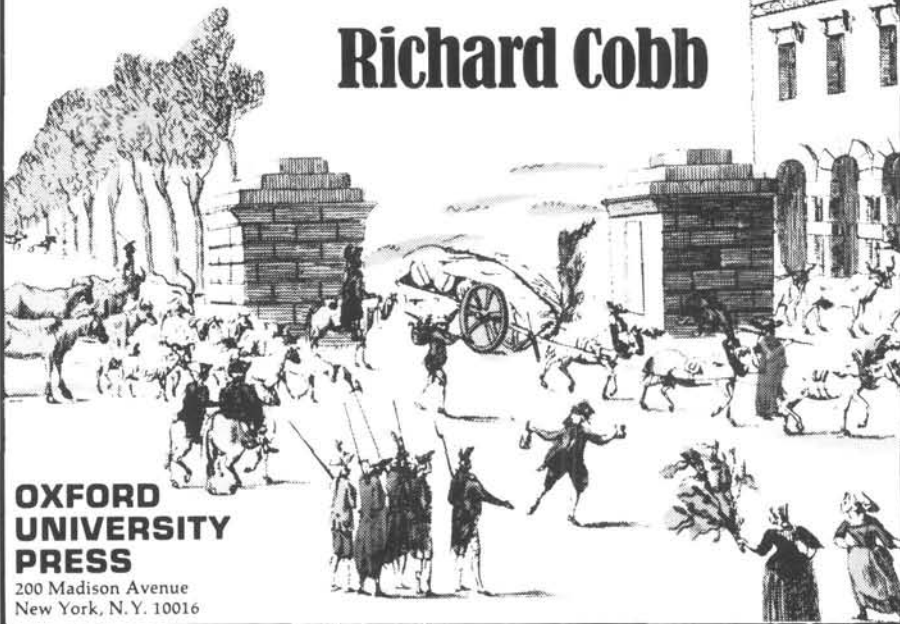


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## Some Models Used by Intellectual Historians

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ROLAND N. STROMBERG

WE MAY START, as an initial theoretical construct, with the conception that ideas evolve dialectically and progressively as one thinker takes up and modifies the thoughts of his predecessor. A simple version of this immanent progressive dialectic of thought supposes a single evolution continuing through history in which one idea answers another, each playing its necessary part and each superior to the preceding: "Slowly the Bible of the race is writ, . . . / Each age, each kindred, adds a verse to it." "The history of knowledge," said Ludwig von Mises, "is the account of a series of successive steps made by men each of whom adds something to the thoughts of his predecessors." Arthur O. Lovejoy called this sort of "Platonic dialogue on a grand scale" the "true procedure of philosophy as science" and held that "the argument gradually shapes itself, through its own immanent dialectic, to a conclusion."<sup>1</sup> Whenever anyone of stature thinks about a problem or issue, he tries to take into account what has gone before, to know "the state of the question," and make his contribution against this background. While personal temperament or social circumstances may cause him to weight some sources over others, he cannot ignore the body of ideas bequeathed as a starting point by previous generations, which constitute the materials of his thought. This simple fact invalidates the view that ideas in any given period are entirely dictated by the physical or economic situation of that epoch. A mechanical Marxism, or any other theory claiming that ideas are exclusively an extrusion of the prevalent socioeconomic order, must itself be subsumed by this pattern; for Marx's ideas were at least in part the result of a long lineage of thought, reaching far back into history. He drew them from such figures as Rousseau, Hegel, Ricardo, and Saint-Simon, who in turn had drawn theirs from others and so *ab initio*.

Yet challenges to the immanent dialectic are numerous. We must quickly abandon the notion that there is one great skein of thought, and that it is spun out steadily. Clearly the very conception of a progression is applicable to some natural sciences as it is not to political, moral, and esthetic ideas,

<sup>1</sup> James Russell Lowell, "Bibliolatres," st. 6; Ludwig von Mises, *History and Theory* (New Haven, 1958), 217; Arthur O. Lovejoy, *The Revolt against Dualism* (New York, 1960), x.

which probably do not progress at all. Even in the former domain, the claim that science is subject to no environmental influences but is based on universally valid laws of logical thought has sometimes seemed doubtful, as for example in World War I.<sup>2</sup> And development is far from even and regular. A science can lie for centuries without making any progress and then suddenly leap forward. It would seem that Copernicus and Galileo made the decisive break-through in astronomy and physics by ignoring much of the most recent science and going back to earlier views thought discredited. John Maynard Keynes similarly went back to an idea thought defunct: economic debate on fundamentals had virtually stopped for a century at Say's Law until circumstances pushed the Cambridge economist to re-examine the old Ricardo-Malthus controversy about sufficient demand and to revise the verdict.<sup>3</sup> Progress in such cases lay in having the vision—or the blindness—to ignore the dialectic or to break up its orderly pattern by reverting to a supposedly superseded statement.

Again, debate may not end but drag on inconclusively; or it may end from exhaustion. There are dead ends in the history of thought, of which Scholasticism, alchemy, and phrenology supply diverse examples. C. Wright Mills objected to the immanent model of intellectual change on the grounds that "very many questions raised and discussed by one generation, or by one circle within it, are neither answered nor even discussed by other circles or later generations."<sup>4</sup> Clearly other factors are at work selecting what questions are to be followed up and what left unanswered, and even which, of many possible answers, are to be given. Much of modern "sociology of knowledge" explores exactly this point. If an orderly dialectical development goes on in some field for a time, this is rather the exception than the rule. Yet it does happen. What of the familiar situation in which pieces of evidence for some theory lie about unrecognized until their accumulation, together with the intrusion of some perhaps adventitious hypothesis, causes their significance suddenly to be realized? Such was the case notably with Darwin's discovery—not a steadily developing argument, yet a cumulative growth.

So the model of dialectical development is only occasionally useful; the historian must apply it with discrimination. It is the same with other such strategies or approaches to history.

THE SOCIOLOGICAL PERSPECTIVE, alluded to above, sees ideas as responses to challenges from the environment or as originating in such challenges—weapons in the struggle for survival, instruments called forth by human needs at any given time. On this view, each epoch or age or generation uses

<sup>2</sup> See Harry W. Paul, *The Sorcerer's Apprentice: The French Scientists' Image of German Science, 1840-1919* (Gainesville, 1972), 35.

<sup>3</sup> See Keynes's essay on Malthus in his *Essays in Biography* (London, 1951), especially page 103.

<sup>4</sup> C. Wright Mills, "The Classic Tradition," in Mills, ed., *Images of Man* (New York, 1960), 10. Hegel himself, of course, was quite aware that development is disorderly and erratic, sometimes standing still and often "pulverized by an infinite complexity of trifling circumstances." *Reason in History*, ed. Robert S. Hartman (Indianapolis, 1953), 88.

what tools it can find to master the challenges with which it is faced. Inherited faiths and long-accepted truths are modified in the light of new conditions. Sometimes they lag behind in their adaptation, but sooner or later those ideas, at least, that are not merely decorative must change or the species (society) will perish. Once established, of course, ideas may outlive the exigencies of which they were born, but cognitive ones must then presumably become vestigial and eventually die. Ideas must prove themselves as instruments in the evolution of mankind. Dialectic makes no great difference; there are any number of possible replies and times to reply; the nature of the reply is conditioned by the practical needs of the day. L. T. Hobhouse claimed that "mind and the world of mind, society, government, the churches, religion, law, are products which have grown up under the pressure of the constant and supreme biological need, and exist only to serve that need."<sup>5</sup> The Marxian or pragmatist counterpart of this Darwinian statement will readily come to mind. Perspectives of this sort see human history as the development of techniques for the more effective conquest of the environment and ideas as weapons in that struggle, whatever form it may take. Historians frequently resort to such hypotheses. They will argue, persuasively, that religious intolerance yielded to tolerance, or laissez faire to social welfare, not because these ideas emerged from the history of thought at this time—in the seventeenth or the later nineteenth century—but because the evolution of society demanded change and new conditions of life forced the alteration of old ideas. This mode of explanation is too familiar to require exposition.

But we need to note that it is far from being universally applicable. Like the others, it only seems to work occasionally or partially. Thus the fascist and communist ideologies were obviously not simply adaptations to the conditions of postwar Europe. Did Einstein fashion the theory of relativity in response to some challenge from the social environment, demanding a new physics? The challenge came from the purely intellectual area of theoretical physics, whose problems affected no important social or economic interest. Such examples suggest the limitations of this perspective, which reductive mentalities often grotesquely exaggerate. Naturalistic explanations of ideas that would reduce them to weapons in the hands of the ruling class, or rationalizations of urges to power or other forms of ego gratification, never do full justice to the data of intellectual history. Though we must see them in a naturalistic light, this light is never enough to illuminate them completely. Careful investigation of even the allegedly most obvious examples of socioeconomic determinism normally refutes the simplification: for example, the claim that Manchester free trade was only a product of the interests of the cotton-mill owners.<sup>6</sup>

<sup>5</sup> L. T. Hobhouse, *Development and Purpose* (London, 1913), 9.

<sup>6</sup> See William D. Grampp, *The Manchester School of Economics* (Palo Alto, 1960), especially the conclusion on page 77 that in the Corn Law struggle "each group acted against its economic interest" as in the light of fuller knowledge we see it today.

In any case, whatever their genealogy, ideas once created take on a life of their own and give birth to other ideas, which impinge upon the environmental world and change it. It is above all necessary, as Raymond Aron has written, "to distinguish the meaning of each sentiment . . . and to respect the specific character of each cultural universe. Religion, science, metaphysics, and art are first of all understood in their authentic existence before the external conditions and social factors of their development are determined."<sup>7</sup>

DIALECTICAL DEVELOPMENT AND SOCIAL DETERMINATION, the two frameworks of intellectual history discussed above, both assume that human concepts develop and change, in the former scheme because of immanent logical factors, in the latter because of social dynamics. Any sane and sound historical work must allow for both, operating at the boundary where they interact. Against both may be placed a perspective that sees thought staying much the same or endlessly repeating previous patterns. *Plus ça change* is more likely to be invoked in the history of ideas than in the history of things. "The more deeply we penetrate into the history of opinions," John Morley observed, "the more strongly we are tempted to believe that in the great matters of speculation no question is altogether new, and hardly any answer is altogether new."<sup>8</sup> From Sextus Empiricus to Nicholas of Autrecourt to David Hume to Ludwig Wittgenstein, we encounter what is at bottom the same skeptical position. There are the eternal Platonists, there was even the ancient Copernicus and the ancient Darwin, Nietzsche went back to Heraclitus. The repetition may be formally different but archetypically the same: millenarianism repeating itself in seventeenth-century religious Antinomians or twentieth-century hippies, Messianism in ancient Christians or modern communists. The Christ of the Fifth Monarchy Men, who came to strike down the rich and usher in the perfect society, appeared on a red horse—the eternal archetype of radical millenarianism. One could readily write a history of human thought on this plan.<sup>9</sup> When we widen our horizon to include the Orient, we find that what seems novelty in the West is familiar elsewhere; we have recently been told that "depth psy-

<sup>7</sup> Raymond Aron, *German Sociology* (Glencoe, 1964), 109.

<sup>8</sup> John Morley, *Diderot and the Encyclopedists* (London, 1886), 1: 3. See also his *Oracles on Man and Government* (London, 1923), 115–16: "The cardinal controversies of human society are few, and they remain curiously fixed. Though the ages use ideas differently, the rival ideas themselves hold on in their preappointed courses."

<sup>9</sup> Albert Salomon has described Jacob Taubes's *Abendländische Eschatologie* (Bern, 1947) as "a comparative typology of the messianic pattern as established by the Jewish prophets, taken up in the revolt of the Franciscans of the thirteenth century, by the founders of German philosophical idealism, and finally by the antithetical doctrines of Kierkegaard and Marx" and that "made possible all theories of progress, reformation and revolution in modern times." *In Praise of Enlightenment* (New York, 1963), 395. American readers may be more familiar with Norman Cohn's *Pursuit of the Millennium* (rev. ed.; New York, 1970) or J. L. Talmon's *Political Messianism* (New York, 1960).

chology" strikes educated Hindus as something they knew ages ago. The game of "nothing new" can be played endlessly, to the embarrassment of those naive enough to allege uniqueness and novelty in the modern situation. Do we not find the same reductionist-materialist position in Skinner and Kinsey that appeared earlier with J. B. Watson and before that in a line extending back to Lucretius? The modern intellectual historian who knows anything about ancient Greece will be checked again and again if he is tempted to identify a novelty. It is even worse if he knows anything of Oriental and other ideas.

So the historian, even if he rejects a cyclical view of history such as offered by the Greeks, Nietzsche, Spengler, Yeats, or others, can choose to show that in the history of ideas a fairly small fund of essential attitudes, if not concepts, is repeated to the end of time—variations on the same notes. He may relate this to asserted constants in human nature that affect ideas: the eternal romantic temperament, for example, the perennial dualism of "drunks and sobers," scholastics and mystics, introverts and extroverts, tender- and tough-minded, or other such classifications. This strategy will not be congenial to most historians, for they know that ideas do change socially and culturally, if not logically or psychologically, and that such social and cultural change is the stuff of life. To say that thought is variations on the same notes is to admit that the music does inexhaustibly modulate. They know that each age is, in some sense, unique and that it is the historian's job to point out this uniqueness. Though, broadly defined, war and tyranny have always existed and the clash of liberty with authority is perennial, no political history contents itself with such empty generalizations. The historian nevertheless cannot allow himself to forget the repetitive element in human thought. Logically, this is the problem of change and continuity, as the first two sections of this essay suggest questions of the origin and role of ideas. Historiographically, it is one of the persistent paradigms of structure.

ANOTHER PATTERN, logically distinct from, though not necessarily in conflict with, those mentioned—dialectical development, social determination, eternal recurrence—is that which claims for each age or generation a distinct *Zeitgeist*. Intellectual historians lean heavily on this, sometimes claiming it as their special province.<sup>10</sup> Long before Ortega y Gasset observed that "at

<sup>10</sup> The prospectus of the Archives Internationales d'Histoire des Idées (The Hague, 1963) spoke of "common modes of thought and feeling from which the major works derive substance," asserting that "there is room for a discipline whose aim is to assess the factors which influence the human mind in one period or in one region." Similarly the German Society for the History of Ideas (Gesellschaft für Geistesgeschichte, founded 1958) assumes that its goal is to try to determine the *Zeitgeist* of each age. John C. Greene believes "the primary function of the intellectual historian is to delineate the presuppositions of thought in given historical epochs." "Objectives and Methods in Intellectual History," *Mississippi Valley Historical Review*, 44 (1957): 59.



any given moment man lives in a world of convictions, the greater part of which are the convictions common to all men who dwell together in their era," Shelley remarked that "there must be a resemblance, which does not depend on their own will, between all the writers of any particular age."<sup>11</sup> Historians of Ranke's time were sure there was "a prevailing tendency of the spirit" that was the key to understanding all the details of any particular historical period. This is hardly a matter, solely, of the next dialectical step in the march of ideas, though it may include youth's rejection of the last orthodoxy; nor is it a matter of the rational needs of the era. It is something elusive, as puzzlingly irrational as the current teen-age taste in music, or the feminine fashions of the 1920s, or why *art nouveau* went with Debussy and Ernest Dowson. It is the culture-creating process, which in traditional societies resulted in local cultures, or fairly stable national "styles," but which in the modern age shifts from generation to generation, among intellectuals no less than others. It makes each era seemingly unique. There are obviously serious objections to a theory of the generations, not least "Which generation?" since a new one is created every year or every moment. The concept of a generation does not, plainly, explain the specific content of ideas—why this rather than that. It merely posits an interrelationship between the ideas of a given period, as part of a nebulous "spirit of the age." But any historian who overlooked this coherence would be lacking in sensitivity and might be led into serious errors. Romanticism, the notoriously indefinable, was just such a *Zeitgeist*, as was the recent surge of student radicalism. Fashions of this sort are often stronger than individuals; that is, a *Zeitgeist* will change a man in mid-air, so to speak, something that happened to many intellectuals between 1790 and 1800 and in the 1930s.

Concepts of periodization, perhaps the average historian's principal framework and about which he engages in endless and rather fruitless debates, are nearly the same thing as *Zeitgeist*. "Renaissance," "Enlightenment," "Romantic era," and so on, unless they are simply names for pieces of time arbitrarily and pointlessly selected, which clearly they are not, stand for the view that in each period a leading idea or theme dominated the culture and gave a special tone to everything. The difficulties such terms have encountered serve to underscore the point that no one framework ever suffices.

It is evident that there can be a dialectic of the *Zeitgeist*, that is, an alteration in the basic mood of a generation, as distinct from the logical progression of an argument or the cumulative development of a science. Reacting against the spirit of scientific positivism and naturalism in general, the generation of ca. 1880–1910 sought a new idealism (which had to be

<sup>11</sup> José Ortega y Gasset's essays on "the generation" are printed in English in his *Man and Crisis*, tr. Mildred Adams (New York, 1958); the quotation is from the essay titled "The Method of the Generations in History," 50. See also Johan Huizinga's remarks in "The Tasks of Cultural History," in his *Men and Ideas* (Cleveland, 1959), 73–74. The quotation from Shelley is from his introduction to "The Revolt of Islam."

different from the old idealism that preceded and gave birth to scientific naturalism), grasping at all sorts of formulations that fitted this larger emotional fixation. "Enlightenment," "Romanticism," and "naturalism" had also been holistic patterns that reacted against the dominant motif of the previous period, more nearly affective designs than specific statements. If we make use of the dialectical model here, we must join it to this concept of unique overall moods belonging to a significant portion of the most sensitive minds in any one generation or time period in a given place. The last qualification suggests limits to this approach: at any given time some minds will be "ahead" of others, there will be those who are still dwelling in earlier *Zeitgeists* from choice or inertia, and the historian must decide which sector he wishes to stress. There will also be local variations.

IN THE HISTORY OF CULTURE another theme that must be taken into account is decay or exhaustion. The intellectual historian may find it puzzling to give an account of its logical connection to the other themes, but he can hardly overlook its presence in his materials. Intellectual formulations are unraveled, one after another, until there are none left, in a kind of intellectual striptease in which veils are one by one cast off until nothing remains except an awareness that all is illusion. The increase in subjectivism and skepticism over the last few centuries testifies to the validity of this process, the final result being that all is created from within, all is "myth" in the most familiar recent sense of that term. Along with the progression of ideas dialectically, or the recurrence of the same ideas, we can detect an ever-growing sophistication that finally becomes a sense of decadence. From Renaissance classicism to baroque to neoclassicism to romanticism to neo-neoclassicism may seem like the mere ebb and flow of the tide, taking us back and forth over the same ground; it may seem like a real development, leading onward and upward even as it spirals; or it may appear as a remorseless exhaustion of options, man wearying successively of each toy until there is nothing left to play with and he dies of boredom, or begins all over again.<sup>12</sup> One might trace a similar pattern in the history of philosophy through the various trials of empiricism, realism, and positivism and, on the other hand, immaterialism and idealism, leading to the present state of near abdication. The sense of decadence felt by so many in the nineteenth and especially in the twentieth century, of all gods dead and all battles fought, the receding wave, overripeness and oversophistication leading to sterility, cannot be ignored by the intellectual historian. In treating the most recent period he must find himself frequently talking about the desperate and fruitless search for values to replace old ones destroyed by modern knowledge, or of a

<sup>12</sup> George Boas's *Rationalism in Greek Philosophy* (Baltimore, 1961) is an account of the degeneration of classical rational thought, which ended by reverting to primitive myth much as Western thought has recently shown signs of doing. Thus there have been cycles of this process.

"recurrent cry of protest or of lamentation before the seemingly irresistible march of a purely mechanistic conception of the world" (the phrase is A. O. Lovejoy's), or of Matthew Arnold's "repeated shocks" that exhaust the energy of strongest souls. Current cultural phenomena of a startling nature may be construed as an explosive human response to the exhaustion of live ideas—as Nietzsche said, men will believe Nothing rather than not believe anything.<sup>13</sup>

The historian will make use of this perspective in at least some areas. He may suspect that the contemporary lament of decadence is itself a persistent archetype; that is, each age imagines a golden age in the past and decries its own corruption; man, in Ortega y Gasset's phrase, being the only creature who yearns for what he never had.<sup>14</sup> In the early eighteenth century Swift worried about decay in Spenglerian terms, seeing George I as Caesar. The "new barbarism" of fragmented and specialized knowledge of which R. P. Blackmur and others have complained in our time was under much the same attack from Rabelais and then Voltaire and then Emerson. But the historian is forced to realize that ages of heroism and faith lurk in the background of the human psyche, and that the modern rationalistic, individualistic, and skeptical mentality leaves some human needs unfulfilled. How else understand such phenomena as "cultural despair," alienation, fascism, the whole position of the modern "intellectual"? (The now characteristic usage of that term has only existed in the last century.) A large number of the most significant modern strains of thought are not to be understood except through an appreciation of their profound rejection of "Faustian" or positivist or mass-democratic civilization.

THE BELIEF OF ST. AUGUSTINE in a City of God and a City of Man has been repeated by others who stood far from his Platonic-Christian position; by Schopenhauer, for example, who wrote that "there are two kinds of history, the history of politics and the history of literature and art," which he chose to describe as the history of will and the history of intellect; the first a tale of woe and terror, the second of pleasantness and serenity. As both thinkers realized, the realms do interact. To the study of this interaction Marxists have contributed a good deal. Sociologies of knowledge and studies of intellectuals in politics may not fully clarify the relationship, but they serve to remind us of a further hypothesis: that theory and practice, thought and action, are different boats which jostle against each other in the stream of history. The record of their collision may be the best key to the history of

<sup>13</sup> Matthew Arnold, "The Scholar-Gipsy," st. 15: "'Tis that from change to change their being rolls; / 'Tis that repeated shocks, again, again, / Exhaust the energy of strongest souls / And numb the elastic powers"; Nietzsche, *The Genealogy of Morals*, essay 3: 1, also 28, in his *Werke*, ed. Karl Schlechta (Munich, 1960), 2: 839, 900: "Will er noch das Nichts wollen als nicht wollen."

<sup>14</sup> Ortega y Gasset, *An Interpretation of Universal History*, tr. Mildred Adams (New York, 1973), 249.

an era. It is no more true that ideas are merely the reflection of economic and social problems and interests than it is that ideas determine the economic-political problems and interests. If we do not hold with Hegel that the Idea underlies and determines the events, or with Marx that material processes ultimately determine the ideas, we may postulate two cities existing side by side, carried in the same matrix but not identical, whose relationship is as debatable as the body-soul connection in the individual organism. One need not postulate an ultimate philosophical dualism to grant that Gramsci's "reality-in-flux and the concept of reality" must at least be distinguished.<sup>15</sup>

We may find relevant Max Weber's almost ferocious conviction that science and politics are two utterly different worlds.<sup>16</sup> The latter is the realm of myth, where prophets and saviors rule, while science, value free and unable to guide life, is the realm of an intellectual aristocracy bent only on understanding. Ethics and power are separated, and the real world is governed by "diabolical powers." The attempt to reduce this diabolical realm to the categories of the understanding, to make it perfectly just and rational, is disastrous when perfectionists try it; they end by slaughtering men for an impossible ideal or by totally confusing both realms. Distasteful as this view may be, it is one that helps explain many historical phenomena. It is perhaps consistent with a dialectical development within the realm of pure thought, but it would view this realm as constantly in a state of tension with the realm of social reality, bouncing off it and being bruised, sometimes igniting explosions in it, but essentially independent. Where Marx saw religion as an expression of ruling-class interest to hoodwink the poor, and Nietzsche viewed it, contrariwise, as a conspiracy of the weak against the strong, as a revolutionary ideology, Weber generally saw no connection between interests and ideas in the inception of the idea; various groups may subsequently select features of it to suit their interests. This seems quite as plausible a proposition as that of the two other great Germans, though it keeps us from a pleasing monism.<sup>17</sup> When a great historian, Lewis Namier, remarks that men's ideas "are but distantly related to reality," we are likely to agree that this is often the case if we equate "reality" with an actual political or social situation: the ideas men wave desperately at this reality-in-flux, and which they force it to fit, seem so utterly at variance with it. Certainly the relationship between thought and action, the concept and the state of affairs, is far more complex than it is made to appear by a crude

<sup>15</sup> Arthur Schopenhauer, "On Some Forms of Literature," in his *The Art of Literature*, tr. T. B. Saunders (Ann Arbor, 1960), 68; Antonio Gramsci, *Selections from the Prison Notebooks*, ed. and tr. Quinton Howe and Geoffrey Nowell Smith (New York, 1971), 378-472.

<sup>16</sup> See especially Weber's famous lectures "Science as a Vocation" and "Politics as a Vocation," both printed in *From Max Weber*, ed. H. H. Gerth and C. Wright Mills (New York, 1958), 77-156.

<sup>17</sup> Studies such as Joan McDonald's *Rousseau and the French Revolution* (London, 1965) seem to bear out this ambivalent usage of ideas by showing how various factions in the French Revolution distorted the philosopher while employing his ideas as political weapons, as they did also with Voltaire. Nietzsche scholars of course argue that Hitler grossly misunderstood Nietzsche, as it appears that Lenin distorted Marx.

economic determinism on the one hand or on the other by such statements as Shaw's "Voltaire, Diderot and Rousseau made Robespierre and Napoleon possible" just as Marx and Wagner made Lenin, Stalin, Hitler, and Mussolini possible.<sup>18</sup> In one or another philosophical formulation, this perspective of conceptual thought over against the external world must be noted by the intellectual historian. Intellectual systems that purport to be descriptions of reality and may be widely accepted as such plainly incorporate large elements of fantasy; the task of the intellectual historian is to reveal this gap between subject and object where it exists.<sup>19</sup> He must understand human conceptual formulations as practically always a more or less inadequate response to an external reality-in-flux that is inexpressibly complex and protean. He must pay attention to the advice recently given to notice "differences as well as likenesses between ideas and life."<sup>20</sup>

THE HISTORY OF IDEAS, then, seems to involve a progression, a decline, eternal sameness, adaptation, a series of unique cultural moments, and a dialogue between interior mind and external reality. No doubt the past, or the accounts given of it by historians,<sup>21</sup> contains other patterns of this sort. It is not claimed that these constitute a complete list, but only that they seem the most persistent and significant for the intellectual historian. That they should be numerous is hardly surprising. Philosophers frequently hold that reality yields to no one formula but must be attacked with varying hypotheses.<sup>22</sup> Historians who insist that the past fits just one framework falsify it, usually in the interest of a fanaticism. (To those who think this point too obvious to require statement, one must reply by pointing to many current specimens of widely read and highly praised works by professional historians.) The patterns I have described are not really inconsistent with each other but are different perspectives on an experienced reality. If, for example, we examine the birth of modern socialism, we can bring in Christ

<sup>18</sup> Lewis Namier, *Avenues of History* (London, 1952), 1; George Bernard Shaw, *Everybody's Political What's What* (New York, 1945), 310.

<sup>19</sup> For a vivid description of man as the only creature capable of creating an inner world of fantasy, see Ortega y Gasset, *Interpretation of Universal History*, 247-48.

<sup>20</sup> In a critical review of Russel B. Nye, *This Almost Chosen People: Essays on the History of American Ideas* (East Lansing, 1966), Rush Welter notes that the work under consideration erred from confusion between events and statements of intellectual conviction, adding that "if the history of ideas is to have any significance at all it must begin by noticing differences as well as likenesses between ideas and life." *History of Education Quarterly*, 7 (1967): 407.

<sup>21</sup> It does not seem to me to matter, for purposes of the historian's theoretical framework of interpretation, whether we view the past as something objectively patterned, which the historian's mind merely uncovers, or something totally chaotic, on which the historian's mind imposes patterns, or, more sensibly, a combination of these two, in which mental categories interact with imperfect external relationships to produce histories that are partly subjective and partly objective. Whatever the epistemological status of our models, they do exist for the historian and serve his purposes. For another discussion of such models or schematic concepts, see Ronald E. Osborn, "Hope beyond History and Fulfillment in History," *Encounter*, 24 (1963): 41-60.

<sup>22</sup> Thus George Boas, *The Challenge of Science* (Seattle, 1965), xv: "I have myself assumed that things and events are interconnected in various ways and not only in one. Therefore I cannot believe that to unravel any one strand of interconnection as if it revealed the essential nature of things is a reliable method of knowing the truth."



on a *red* horse without excluding a dialectical debate that runs at least through the eighteenth and earlier nineteenth centuries and without excluding the impact of great changes in society (not the *only* source of socialism), the intangible spirit of the age in the Romantic era, and interactions between theory and practical experience. These were all obviously involved; and also in some sense, not inconsistent with the progressive development of thought, socialism reflected a decay, a falling away from cultural wholeness after an exhaustion of older options; it was a self-conscious attempt to recapture the lost natural community. The past in fact allows us to use all these patterns, with profit in the form of fuller understanding. We must hold them all in some sort of balance to understand cultural and intellectual phenomena fully.

It has been rather widely held that a "crisis" exists in historiography because the simpler, nineteenth-century faiths have been lost, whether those of progress, of social science (Darwinian or Marxian), or of historical idealism. We cannot wholly restore any of these faiths, but it may be possible to use them all as aids to the understanding. We can proceed to a more complex historiography, which is not necessarily a chaotic one. Historians have of late been advised to examine their vocabulary and use terms consistently. They would be equally well advised to examine their patterns and use these systematically. History cannot be written from theory alone, but it can hardly be written without theory; there is an important sense in which theory precedes history.

Among those who have found the patterns of history baffling in our time, one can cite not only H. A. L. Fisher, who aroused Toynbee by finding only "one emergency following upon another as wave follows upon wave . . . the play of the contingent and the unforeseen," but also that wayward novelist John Barth, author of *The Sot-Weed Factor*: "The Poet Wonders Whether the Source of Human History is a Progress, a Drama, a Retrogression, a Cycle, an Undulation, a Vortex, a Right- or Left-Handed Spiral, a Mere Continuum, or What Have You. Certain Evidence is Brought Forward, but of an Ambiguous and Inconclusive Nature."<sup>23</sup> This uncertainty about history very probably is, as the case of this novelist of the Absurd suggests, a leading source of the cultural and intellectual confusion of our time, reaching into all areas of life. It is up to historians to show that we can restore order. We may then partly satisfy those numerous intimations that as a branch of knowledge history has never yet fulfilled its potentiality, the feeling that led Ortega y Gasset to declare, "God will not forgive the historians."<sup>24</sup> But Barth's Poet must get over his dogmatic conviction that history must be one or another of these various things. It is almost certainly all of them.

<sup>23</sup> H. A. L. Fisher, *A History of Europe* (London, 1935), 1: vii; Arnold J. Toynbee, *A Study of History* (London, 1939), 5: 414; John Barth, *The Sot-Weed Factor* (New York, 1967), 679.

<sup>24</sup> Ortega y Gasset, *Gesammelte Werke* (Stuttgart, 1956), 3: 364; see also Karl J. Weintraub, *Visions of Culture* (Chicago, 1966), 285.

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## Agrarian Capitalism or Seigniorial Reaction? The Northwest of England, 1500–1700

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ANDREW B. APPLEBY

THE GREAT DEBATE on early modern rural capitalism continues. Recently Professor Eric Kerridge has challenged R. H. Tawney's eloquent but dated description of a helpless peasantry uprooted by the cruel workings of agrarian capitalism in sixteenth-century England. Because their debate provides the framework for this article, I will try here to reduce the subtle and complicated arguments of both men to a few sentences. Put broadly, Tawney maintained that landlords utilized every possible tactic to avoid financial loss during the long price inflation of the sixteenth century. Faced with fixed returns on rents, some landlords evicted their customary tenants<sup>1</sup> and enclosed the common arable fields to turn them into profitable sheep runs. Other lords pushed up their tenants' entry fines to offset diminished real returns on rent. The first tactic depopulated the countryside, creating the mass of vagabonds that marked Tudor England. The second siphoned off the profits from the price rise in foodstuffs into the pockets of the lords, leaving the tenants impoverished. In both cases, Tawney argued, legal restraints on the lords were inadequate to protect the tenants. These tactics appeared to him as evidence of a new entrepreneurial spirit that substituted a bald cash nexus for the older, more humane mutual responsibility that had prevailed between tenant and lord.<sup>2</sup>

Kerridge, on the other hand, depicts capitalism as a positive good that brought about a revolution in agriculture. Capitalism in agriculture was not achieved at the expense of the peasants, but with their cooperation. In fact the agricultural revolution could not have taken place unless the customary

I would like to thank J. O. Appleby, R. P. Brenner, and A. J. Slavin for helpful criticism of this article. My thanks also to the Cumbria County Council Archives Department and to the Department of Palaeography and Diplomatic of the University of Durham for their many kindnesses.

<sup>1</sup> Customary tenants are here defined broadly to include all those holding by local customary laws. The freehold tenants were fewer in number and were not vulnerable to the same pressures that the lord could bring against the customary tenants.

<sup>2</sup> R. H. Tawney, *The Agrarian Problem in the Sixteenth Century* (1912; reprint, New York, 1967). Tawney of course saw the gentry as the agents of this entrepreneurial spirit. For the beginning of that Tawney-generated debate, see his "The Rise of the Gentry, 1558–1640," *Economic History Review*, 11 (1941): 1–38.

tenants had security of tenure. They would not have invested their capital or labor in the necessary innovations and improvements without the assurance that they, or their heirs, would enjoy the profits of that investment. Kerridge further argues that the lord could neither evict the customary tenant nor raise his entry fine to confiscatory levels. Nor would the lord do so, even if he could, for he needed the tenant, as the tenant needed him. Together—as partners—they transformed English agriculture.<sup>3</sup>

Since their observations range over all of England, both men necessarily minimize local variations in their conceptual models. In this article I hope to amplify—and modify—their descriptions of agricultural change in the northwest.<sup>4</sup> Developments in the two isolated counties of Cumberland and Westmorland point up shortcomings in both men's models and suggest that each presents a true but incomplete picture of English agrarian society. The picture needs elaboration in the two overlapping areas of enclosure and tenant-lord relations. In these northwestern counties geographical circumstances brought about a form of enclosure that encouraged overpopulation, with results more unpleasant than the depopulation usually associated with enclosure. In their relations with their lords, the tenants enjoyed a degree of protection against exploitation until 1603, when security of tenure was undermined by the union of the Crowns. After 1603 the lords systematically squeezed their tenants—much as Tawney alleged—extracting higher fines in lieu of increased rents. The customary tenantry of Cumberland and Westmorland did not have the security of property that Kerridge argues was typical of England. But paradoxically this very lack of security underscores the validity of Kerridge's major thesis that capitalism was beneficial, rather than the monster Tawney made it. The absence of capitalism seems inextricably tied to the rural backwardness and impoverishment of the region. The two counties, and perhaps the other northern upland counties, may be the exception that tests Kerridge's rule. For this reason, I think, the agrarian conditions of the northwest are of more than local interest and may provide a further insight into early modern agrarian societies.

IN SIXTEENTH- AND SEVENTEENTH-CENTURY Cumberland and Westmorland, enclosure took the form of encroachment on the common pasture rather than the more-celebrated form of conversion of common arable fields to pasture.<sup>5</sup> In the northwest there was little reason to convert arable to pasture

<sup>3</sup> Eric Kerridge, *Agrarian Problems in the Sixteenth Century and After* (London, 1969), and *The Agricultural Revolution* (New York, 1968). Together these volumes represent Kerridge's basic view of English agrarian society and the legal framework within which it operated.

<sup>4</sup> For specifics about the northwest, see Kerridge, *Agricultural Revolution*, 160–65, 171–73, and his *Agrarian Problems*, 43–45, 58–60. Tawney had little data from the northwest but much from the northeast. See *Agrarian Problem*, 189–91, 299, 303–05.

<sup>5</sup> Enclosure, in its various forms, is discussed thoroughly in Joan Thirsk, "Enclosing and Engrossing," in Thirsk, ed., *The Agrarian History of England and Wales*, vol. 4: 1500–1640 (Cambridge, 1967), 200–55.

and good reason to convert pasture to arable. Pasture—or “waste,” as it was often called—was plentiful, in many manors making up the vast majority of the land area.<sup>6</sup> Arable, on the other hand, was very scarce in the Cumbrian and Pennine highlands and far from plentiful in the lowlands of the Solway Plain and the Eden Valley. Population growth during the sixteenth century outran the productive capacity of the “old” arable lands and brought a continued conversion of pasture land to arable to make up the deficit. In the highlands this involved taking in bits of pasture and planting them in grain, in the hope of eking a crop or two out of the poor soil before it became exhausted and had to be returned to grass again. In the manor of Rogerside, in the Cumbrians, nine of the twenty-nine customary tenements contained land taken in from the common pasture, according to a survey taken in 1570. In Buttermere, out of twenty-one tenements, twelve were made up partly of converted pasture. At Braithwaite, the survey shows fifteen separate parcels of land enclosed from the waste.<sup>7</sup> Again and again the surveys taken in 1570 and 1578 of the extensive Percy holdings in western Cumberland show such intakes, which were added to the holdings of established customary tenants or given to otherwise landless men who tried to scratch a living from their small enclosures.<sup>8</sup> The same process is observable in the Pennine foothills. To cite only one example, of the thirty-nine tenants in the manor of Cumwhitton in 1574, seven held nothing but enclosed waste land.<sup>9</sup>

The taking in of small parcels of the common pasture in the highlands by certain of the customary tenants probably did not harm other members of the community. Even squatting on the waste, as at Cumwhitton, would have had little adverse effect, although the squatters’ lands came from the common pasture. The upland pastures were vast, largely unsuitable for cultivation, and chances were slender that enough land would be converted to private use to reduce seriously the customary tenant’s pasturage. But occasionally there was opposition. In 1615 Lord William Howard erected four cottages on the North Moor in his barony of Gilsland and rented them to four poor men. Howard’s customary tenants promptly pulled the cottages down, expelling the new men.<sup>10</sup> The North Moor at that time was a great

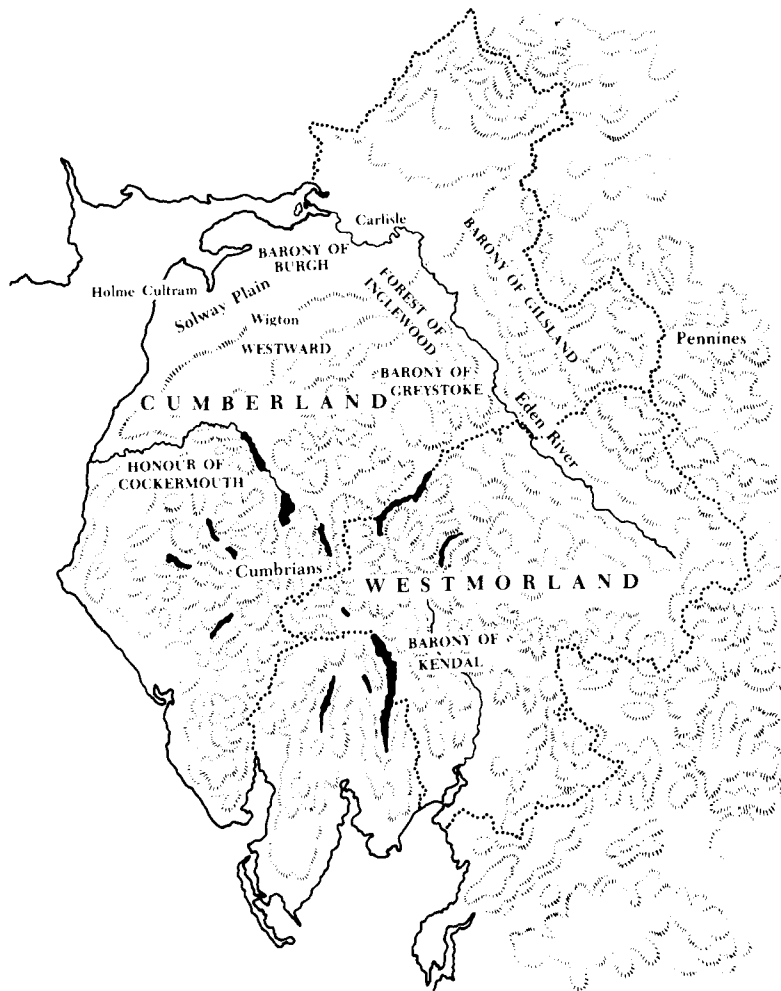
<sup>6</sup> For example, the upland manor of Matterdale was eighty-eight per cent common pasture, or “waste,” in 1589; in 1604 Bewcastle was ninety-two per cent waste. See R. S. Dilley, “Common Land in Cumberland, 1500–1850” (M. Litt. thesis, Cambridge University, 1972), 69. Students of agrarian conditions in Cumberland will find this meticulous study by a historical geographer indispensable.

<sup>7</sup> Survey of 1570, Public Record Office, London (hereafter PRO), Exchequer Miscellaneous Books, series 1 (hereafter E. 164), 37, fols. 43–45, 52v–54v, 35v–39v.

<sup>8</sup> The survey of 1570 is cited directly above. The survey of 1578 is in the Leconfield Papers at Cockermouth Castle, administered by the Cumbria County Council Archives Department, Record Office, the Castle, Carlisle (hereafter CRO).

<sup>9</sup> Survey of Gilsland of 1574, incorrectly dated 1558, xerox copy in CRO.

<sup>10</sup> Equity proceedings, PRO, Court of Star Chamber, Proceedings, James I (hereafter Sta. Cha. 8), 164/4.



CUMBERLAND AND WESTMORLAND PLACE NAMES

barren unpopulated waste,<sup>11</sup> and it seems unlikely that the customary tenants felt such a minor loss of pasture. Indeed the tenants may not have had the right of pasture there. Why would they object to a few squatters on a distant moor in 1615 when the inhabitants of Cumwhitton, in the same barony, had not opposed squatting on their common prior to 1574? Apparently the tenants were offended by Howard's taking the initiative in settling these men. Howard and his customary tenants were then involved in an acrimonious lawsuit over the status of customary tenure, and one of the points at issue was whether Howard could establish tenants at will, that is, tenants who would occupy only at his sufferance.<sup>12</sup> The older tenants

<sup>11</sup> Estate accounts, Howard of Naworth Papers, C173, Department of Palaeography and Diplomatic, University of Durham.

<sup>12</sup> Miscellaneous documents, CRO, Mounsey-Heysham Papers, 6, pp. 2-8; equity proceedings, PRO, Sta. Cha. 8/161/16 and 8/162/13.

almost certainly regarded the new cottagers as setting a harmful precedent; for this reason they expelled them. Opposition to encroachment on upland pasture was rare, however, judging by the great number of encroachments and the absence of tenant opposition. Most encroachments must have been accomplished amicably, with the encroacher receiving a bit of land and the lord a few pennies in additional rent.

Pasture was less plentiful in the lowland manors than in the highlands, and there were fewer encroachments. The excess population, however, found land in the great forests. A commission in 1578 reported 178 different encroachments within the Forest of Inglewood, a vast, ill-defined Crown property that stretched north from Penrith almost to Carlisle. The majority of the encroachments were very small, an acre or less, and appear to have been held by poor squatters who had no other land. Customary tenants of adjoining manors had also taken in small parcels of forest land, usually by setting a hedge a few yards into the forest. Finally local gentry had made other, larger encroachments, which they then rented to poor men. Mr. William Skelton, for example, had an encroachment with six houses on it, each of which was rented to a separate family. Eleven families lived on the land enclosed by Mr. Thomas Denton. The Crown accepted the intruders and instructed the commissioners to admit them as copyhold tenants, provided they pay an entry fine "according to the custume of the Contrye" and agree to the rent the commissioner set.<sup>13</sup>

In 1619 a survey of the forest revealed 757 different encroachments, including 602 houses.<sup>14</sup> Some of these had been confirmed by the commission of 1578, but many others were more recent. Although the surveyors in 1619 arranged their data somewhat differently than had the commissioners of 1578, a comparison of the two indicates that many more people inhabited the forest in 1619 than forty years earlier. The pattern of holdings remained the same: many squatters with tiny holdings, some customary tenants of neighboring manors who had enlarged their holdings at the expense of the forest, and a few local gentry who sublet the land they had enclosed. In addition to providing more arable land for customary tenants of nearby manors, the forest continued to be an outlet for the expanding population, a place where landless men could find their own small plots of ground.

The Forest of Inglewood was enclosed piecemeal through the initiative of those who encroached. The Crown only later confirmed what had been done by private action. On the other hand, enclosure of a large part of the Forest of Westward, a Percy holding, was accomplished at one time, in 1569, and seems to have been initiated by the steward of the forest. A survey of 1570 shows ninety-four tenements and six cottages in the forest. The men

<sup>13</sup> Crown estate accounts, PRO, Exchequer Office of the Auditors of Land Revenue, Miscellaneous Books (hereafter L.R. 2), 213, fols. 67-74.

<sup>14</sup> Survey of Inglewood Forest, CRO, Mounsey-Heysham Papers, Penrith-Inglewood Survey, 1619.



receiving the land came from diverse economic and social levels, as was the case in Inglewood, but there were fewer poor men who had no other holdings. Prominent local gentry received land: one, John Briscoe, had extensive holdings in several nearby manors; another, Anthony Barwis, was steward of the Honour of Cockermouth and a man "of great worshipp in those partes."<sup>15</sup> Both these men undoubtedly sublet their new holdings to others, probably to men without other land. The majority of the individual enclosures in the forest went to established customary tenants of nearby Percy manors. The example of John Adamson is typical. He received a tenement with four acres of land in the forest. At the same time he also held a cottage with one and one-half acres in the manor of Wigton and another tenement with five acres in the manor of Woodside. Westward, Wigton, and Woodside are about five miles apart, and Adamson probably let his new holding in Westward rather than working it himself. Possibly he settled a younger son there.<sup>16</sup>

I have given these details to show that the large majority of the encroachments within the forests ended up either directly or indirectly in the hands of men without other land. In some cases the squatters held directly from the lord of the forest, in others the land went first to a local gentleman or an established customary tenant and was then sublet by him to a landless man. Far from causing depopulation, this type of enclosure by encroachment held population on the land by creating numerous marginal agricultural holdings. This took place in the highlands, where squatters settled on the waste, as well as in the lowland forests. I have argued elsewhere that Cumberland and Westmorland suffered two, perhaps three, terrible famines at the end of the sixteenth and beginning of the seventeenth centuries.<sup>17</sup> If my interpretation is correct, the region was overpopulated in a Malthusian sense. The tiny holdings that proliferated through enclosure surely contributed to this overpopulation and resulting starvation. Whether the poor squatter or subtenant would have fared better if he had been forced to migrate is, of course, unanswerable, but it is at least possible.

Depopulation was considered an evil both by sixteenth-century writers and by Tawney. When it took place it undoubtedly gave rise to serious social and personal dislocations. But overpopulation was also an evil, especially if it led to starvation. No one was to blame; both the squatter, or subtenant, and the lord wanted the land enclosed. Enclosures held out to every man the hope of finding his own land rather than migrating to an unknown region and an uncertain future.

In addition to creating a large group of vulnerable smallholders, enclosure in some instances worked immediately and directly against the

<sup>15</sup> Survey of 1570, PRO, E. 164/37, fols. 77-86, 264.

<sup>16</sup> These manors are surveyed in *ibid.*, fols. 57v-75v. A comparison of surnames shows that many men owned land in more than one manor and also held forest land.

<sup>17</sup> Andrew B. Appleby, "Disease or Famine? Mortality in Cumberland and Westmorland, 1580-1640," *Economic History Review*, 2d ser., 26 (1973): 403-32.

poor. Shortly after the Forest of Westward was enclosed in 1569, three to four hundred men from nearby manors entered the forest and pulled down the enclosures. They came from the manor of Thursby, a former Dacre property now in Crown hands; from the barony of Dalston, held by the bishop of Carlisle; and from various Percy manors adjoining the forest.<sup>18</sup> The bishop of Carlisle wrote the Privy Council complaining about the enclosure and noted that the men who had participated in the tumult were poor men who depended on the forest to pasture a few animals, without which they could not survive.<sup>19</sup> The proceedings of the court of oyer and terminer held at Carlisle confirmed that the rioters were "verie pore Servantes & labourers." The proceedings also revealed that they were tenants of the various manors, holding tiny tenements or cottages.<sup>20</sup> By combining their earnings as laborers, their slim crops from small holdings, and their rights to pasture their animals in the forest, they had been able to make a scant living. Without the pasture, they could not.

The enclosure of the Forest of Westward raises a problem Kerridge is inclined to minimize. He emphasizes that none of these tenants had a legal right to pasture in the forest—and he is correct. He is also correct in saying that each customary tenant—and the rioters appear to have been customary tenants—had security of tenure on his small tenement or cottage holding. Each tenant also enjoyed the right of pasture on his manorial common, although the number of animals he could pasture there may have been severely limited. In other words, the tenant retained all his old rights and privileges. But this was no longer enough. The great population surge of the sixteenth century had added too many men to the rural structure, straining it to the point of collapse.<sup>21</sup> Customary tenements had been fragmented; new tenants at will had been added. All competed for limited land and pasture. The better-off tenant, like Adamson, gained from this competition by receiving a part of the newly enclosed lands, although at the cost of some pasturage. But the poor tenant gained nothing. If he was to subsist he needed legal rights that went beyond customary, traditional protection.

Besides worsening the economic position of many customary tenants, population growth created a vast new army of men who had little legal protection of any sort. Squatters were sometimes enrolled as copyhold tenants, as in the Forest of Inglewood. More often they remained tenants at will, without legal recourse if they were evicted. All the tenants within

<sup>18</sup> C. M. L. Bouch and G. P. Jones, *A Short Economic and Social History of the Lake Counties, 1500–1830* (Manchester, 1961), 77–78; indictment of prisoners, PRO, State Papers Domestic, Addenda, Edward VI to James I (hereafter S.P. 15), 14/87 3.

<sup>19</sup> Richard, bishop of Carlisle, to Lord Burghley, Oct. 20, 1571, in Robert Lemon and M. A. E. Green, eds., *Calendar of State Papers, Domestic, Addenda 1566–1579* (London, 1857–), 367.

<sup>20</sup> Report of the commission of oyer and terminer, PRO, S.P. 15/14/87 1.

<sup>21</sup> The somewhat unreliable demographic evidence suggests that rural population grew rapidly until approximately 1600 and then began a slow decline that lasted through the seventeenth century. See Andrew B. Appleby, "Population Crisis and Economic Change: Cumberland and Westmorland, 1570–1670" (Ph.D. dissertation, University of California, Los Angeles, 1973), 21–35; and Bouch and Jones, *Short Economic and Social History of the Lake Counties*, 79–83.

Westward fell into this category. For some, such as the wealthy gentry or the favored customary tenants, this was not a threat to their livelihoods; by granting them lands "at will," the Percys gained local good will and supporters. It was merely time-honored "goodlordship."<sup>22</sup> For the subtenants who actually worked the land, it was another matter. If they could not come up with the rent demanded each year, they faced eviction. And given the excess population, there were always candidates for the holding, willing to live at subsistence levels rather than migrate.

These comments apply to the northwest, where the excess population stayed on the land. The rest of England knew a similar demographic expansion<sup>23</sup> but had less moor and forest to absorb the surplus people. Probably they drifted, making up the swarms of masterless men that so impressed sixteenth-century observers. Because contemporaries imperfectly understood the population dynamics of the century, they assumed that the vagabonds were a result of enclosure and depopulation. It seems more likely that they were merely superfluous—extra men and women who could find no place in an economy that had largely been fashioned earlier and had not yet expanded sufficiently to fit them all in. If this is so, both Tawney and Kerridge have emphasized different aspects of sixteenth-century society. Tawney, always sensitive to injustices but unaware of the dimensions of population growth, mistakenly accepted the causes given in the sixteenth century for social dislocation, namely, the eviction of customary tenants. Kerridge sniffed out the errors in Tawney's legal argument but does not acknowledge the true extent of miserable and vulnerable groups in society. The existence of these unfortunates, however, in no way negates Kerridge's argument that an agricultural revolution took place. Had it not, it seems probable that starvation would have faced many early modern Englishmen, rather than just the poor of isolated and backward northern counties.

TO THIS POINT I have mentioned the customary tenants only in connection with enclosures. We have seen that some benefited at the expense of others. Let us now consider other relations between the customary tenants and the lords, again within the general context of the Tawney-Kerridge debate. The following interrelated questions are the fundamental points at issue. First, did the customary tenant have security of tenure in his lifetime, and did his heir have the right to inherit the customary holding on his death? Or could the lord evict the tenant or prevent the entry of the heir? Tawney stressed the tenant's lack of security; Kerridge stresses its presence, arguing that without security the capital improvements necessary for an agricultural

<sup>22</sup> See M. E. James, *Change and Continuity in the Tudor North* (York, 1965), 24–25.

<sup>23</sup> Julian Cornwall estimates a population increase for England of about sixty-three per cent between 1522 and 1603. "English Population in the Early Sixteenth Century," *Economic History Review*, 2d ser., 23 (1970): 32–44. The English population expansion, in turn, was but part of the general European trend.

revolution would not have been made. Second, did the lords push up rents and entry fines to grasp for themselves any profits arising out of the long upward movement in food prices? Here again, Tawney argued that this happened, and Kerridge has countered by saying that, without some incentive, the customary tenants would not have devoted the capital and labor necessary for an agricultural revolution. It is clear that these two questions are different sides of the same coin. Security of tenure and fixed, or "reasonable," rents and fines meant that the tenant could benefit from his investment of time and capital. Insecurity of tenure and rackable rents and fines meant that the lord could skim off the profits if he wished.

Before the union of the Crowns in 1603, each customary tenant in Cumberland and Westmorland had two tenurial ties, one directly to the Crown and another to his manorial lord. The tie to the Crown obliged all customary tenants to serve in defense of the border. Specifically the tenants were "upon one howers warning [to] serve upon the . . . borders of Scotland with ther Armour . . . at ther owne costes & charges from tyme to tyme during the pleasure of the Lord Warden of ye said marches."<sup>24</sup> Despite the burden this involved, the customary tenants commonly referred to border service as the "anncient & lawdable custom of tenant right," for it provided them with a weapon in their disputes with their manorial lords. The Crown did not wish to see the inhabitants of the border counties so impoverished that they could not provide themselves with horses and arms. Unprotected by an armed tenantry, the north would be open to invasion or raiding by the Scots. Under Elizabeth the customary tenants could—and did—appeal to the Crown for protection against excessive exactions by their landlords. And, as we shall see, the Crown sometimes took their side.

The tenant's second, manorial, tie obliged him to pay various rents and fines, sometimes a heriot, to contribute labor services called "boon days," to attend the manorial courts—in short to perform the myriad services and make the payments demanded of tenants throughout England. In return the tenant had the right to occupy and work his customary lands, pasture his cattle on the waste, cut underwoods, dig peat, and so forth.<sup>25</sup> The details need not concern us here; we only need to know if the lord evicted tenants or raised rents and fines.

Individual tenants were evicted from time to time, of course, for non-payment of rents or fines, or for felony, but, with the exception of Thomas Lord Wharton's eviction of a number of tenants at Ravenstonedale to create a park,<sup>26</sup> there is no evidence that entire groups of tenants were dispossessed

<sup>24</sup> Equity proceedings, PRO, Court of Requests, Proceedings (hereafter Req. 2), 165/165. The phrasing was formulaic, almost identical wording appearing in numerous other descriptions of tenant right.

<sup>25</sup> The customs of tenant right can best be found in Annette Bagot, "Mr. Gilpin and Manorial Customs," *Transactions of the Cumberland and Westmorland Antiquarian and Archaeological Society*, n.s. 62 (1962): 224–45.

<sup>26</sup> Equity proceedings, PRO, Req. 2/188/14; James, *Change and Continuity in the Tudor North*, 25, 25n.

during the sixteenth century in Cumberland or Westmorland. In part this may have been because the Crown opposed any reduction in the armed tenantry; in part it certainly was because there was little economic reason for large-scale evictions. As I have already pointed out, upland pasture was plentiful, making eviction to convert to sheep runs unnecessary. Moreover no lords evicted their tenants to obtain their lands for large-scale grain production. Most landlords stayed out of the grain market, living off rents and usually letting rather than cultivating their own demesne land.<sup>27</sup>

Nor did the lords attempt to raise the tenants' rents. Perhaps the lord recognized that his rentals and surveys would be strong evidence against him in the event of litigation. But entry fines offered more opportunities for manipulation. These fines were due on the change of lord or tenant to reconfirm the tie that bound the two together. Fines fell into two categories: a "general" fine on the change of the lord and a "particular," or "dropping," fine on the change of the tenant.<sup>28</sup> Fines were expressed as a multiple of the customary, old rent and were given in pence, each penny being a multiple of the rent. For example, a fine of 20d. did not total 20d. but was twenty times the old rent. On most manors fines had not been customarily established at any precise multiple of the old rent. Instead they were arbitrary, to be agreed upon between the lord and the tenant. The lord could set the fines at whatever level he wished, subject only to the vague requirement that they be "reasonable."

During the course of the sixteenth century, private landlords demanded progressively greater fines of their tenants. The Percys apparently asked their customary tenants to pay fines of one and one-half years' rent in 1530–31, a level somewhat higher than that prevailing toward the end of the fifteenth century.<sup>29</sup> Tensions over fines flared up during the Pilgrimage of Grace in 1536 when attacks on landlords were attributed in part to excessive fines.<sup>30</sup> According to one witness, the commons forced the landlords at one point to agree to moderate fines. When he wrote Thomas Cromwell in 1537 suggesting clemency for most of the rebels, the duke of Norfolk stressed that the tenants had been fined "marvellously sore in time past."<sup>31</sup> We do not know how frequent or onerous the fines were, but other evidence indicates that fines became heavy and frequent on many estates only later in the century. The queen's surveyors, Edmund Hall and William Humberston,

<sup>27</sup> Exceptions were the Lowthers of Lowther, Westmorland; the Dalstons of Dalston, Cumberland; and after 1628 Lord William Howard of Naworth Castle, Cumberland. The Percy demesnes were leased out at an early date. See J. M. W. Bean, *The Estates of the Percy Family, 1416–1537* (Oxford, 1958), 12–15.

<sup>28</sup> Bagot, "Mr. Gilpin and Manorial Customs," 229.

<sup>29</sup> Bean, *Estates of the Percy Family*, 58–59.

<sup>30</sup> Instructions to the duke of Norfolk and the earl of Sussex, confession of Barnarde Towneley, examination of William Colyns, in R. H. Brodie, ed., *Letters and Papers, Foreign and Domestic, of the Reign of Henry VIII* (London, 1920–32), vol. 12, pt. 1, pp. 51, 136, 301, 415.

<sup>31</sup> Examination of Robert Thompson, letter from the duke of Norfolk to Thomas Cromwell, in *ibid.*, 304, 226.

noted the change that had taken place by 1570 in the Percy Honour of Cockermouth, the single largest lordship in the region.

The tenants thought themselves well pleased & in good estate & albeit their ffermeholds were but small yett the Comons were great & . . . the Tenants were able to live . . . till nowe of late yeares the greedines of the Lordes hath been such & their practices soe horrible by making Conveyance & devises of their Lande to cause the poore tenants to make ffine sometimes once in two three or ffoure yeares or more [and] the poore tenants are soe raunsomed as they are neither able to live & mainteine their families.<sup>32</sup>

The mechanism of "Conveyance & devises" of the land, to force the tenants to pay a fine, is not clear from the context. It is also not clear just who the "Lordes" were, that is, whether the reference is to the Percys or to lesser lords holding of the Percys.

A complaint lodged in the Court of Requests in 1584 against Sir Edward Herbert and his wife Mary by their tenants in the manors of Loweswater, Brackenthwaite, and Thackthwaite provides clarification. The tenants claimed that the Herberts had twice sold the manors within the previous fourteen years and feared that they would continue to sell them in the future, each time demanding a general fine.<sup>33</sup> Evidently the sales were collusive, a transfer of the land on a pretended sale to another gentleman who then "sold" the manors back again. Collusive sales of this type seem to have been a commonplace among the gentry of the honour, although nothing indicates that the Percys used the subterfuge on their own numerous manors. We do not know how the tenants fared in their suit, but a few years later Chancery ruled (39 Elizabeth) that a general fine was not owed if a lord sold a manor. Sir Edward Coke, writing of this decision, noted that the change of the lord "ought to be by Act of God, otherwise no fine can be due," or else the tenants "may be oppressed by multitude of fines."<sup>34</sup> Perhaps this principle had not yet been established. The intent of the lords in the Honour of Cockermouth is clear, however; they wanted to milk their tenants, and, if the queen's surveyors were correct, some, at least, had been successful.

Complaints about higher fines were frequently heard toward the end of the century. In the Herbert case mentioned above, the tenants also objected to paying ten years' old rent for a fine. Before the manors descended to Lady Mary from her father, they claimed, fines were set at two years' rent. The great and frequent fines, the tenants alleged, would "utterly undoe and begger" them, and they would be unable to do their border service.

In 1589 the bishop of Carlisle was a defendant in a suit brought in Requests by one of his tenants in Dalston, George Blaymyre, who said that his

<sup>32</sup> Survey, Lands of the earl of Northumberland, PRO, Commissioners of Forfeited Estates, Papers (hereafter F.E.C. 1), N12, pp. 2-6.

<sup>33</sup> Equity proceedings, PRO, Req. 2/186/157.

<sup>34</sup> Quoted in Kerridge, *Agrarian Problems*, 147.



ancestors had always paid a fine of two years' old rent but that the bishop was now demanding a "great intollerable and unaccustomed fine." Blaymyre had refused to pay the fine, and the bishop was seeking to evict him by an action of *ejectio firma* at common law. Blaymyre went on to say that he could not plead his tenure of tenant right as a defense in a common-law action because tenant right was peculiar to the northern counties and not "common" to all of England. His only recourse, he added, lay in a court of equity. He had turned to Requests because he could not expect a fair hearing in the Council of the North as the bishop was a member.<sup>35</sup>

These same themes run through the bulk of cases heard in the courts of Chancery and Requests. The fine demanded by the lord was "extreame & outrageous . . . utterly contrary to the ancient custome" and would cause the "utter undowing & impoverysing" of the tenants. Because their tenure was peculiar to the north, they had no defense against common-law eviction actions and had to seek equitable justice. All the tenants' pleas emphasized that they would be unable to defend the border if the lords' demands were not moderated.<sup>36</sup>

It is clear that the equity courts were at times sympathetic to the tenants' complaints.<sup>37</sup> Exactly how often it is impossible to say. But there existed a modicum of protection for the customary tenant if he could bring his case before an equity court. No doubt the court decisions were often compromises, raising the fine somewhat but keeping it "reasonable" in hopes of leaving the tenant prosperous enough to meet his obligation to defend the border. London was far away, however, and the tenant poor, and even equity proceedings were time consuming and expensive. Probably only the most flagrant cases were heard and perhaps adjudged in the tenants' favor.

Pleas in court cases are naturally biased; the tenants' description of their dismal lot has to be viewed skeptically. Their complaints were substantiated, however, by other reports from the north. An unsigned letter in 1591 to the Privy Council gave the following reasons for the decline in the number of horsemen available for border service. First, the "Principall cause" was that Cumberland lords exact "as greate or greater fynes than they doo in the Southe, besides the contynuall services they doo putt their Tenants unto for all mannor of . . . worke whatsoever." Second, the estates formerly occupied by gentlemen are now held by "meane men" who so squeeze their tenants "as they are scant able to find themselves and their families meate and drinke." Third, Crown lands are leased to men who "take so unreasonable fynes as they [the tenants] are not able to buye a good horse, or havinge

<sup>35</sup> Equity proceedings, PRO, Req. 2/178/52.

<sup>36</sup> The quotations are from the depositions in PRO, Req. 2/165/165. Also see the following equity cases: PRO, Req. 2/211/5; Req. 2/233/10; Req. 2/164/27; Chancery Proceedings, series 2 (hereafter C. 3), 284/34; and Sta. Cha. 8/97/15.

<sup>37</sup> References to decisions in depositions, PRO, Req. 2/218/42; C. 3/284/34; Exchequer, King's Remembrancer, Depositions taken by Commission (hereafter E. 134), 8 Chas 1/M16; E. 134/35-36 Eliz/M23.

one is not able to keepe him." Leasing a manor to such a man "does begger all the Tenants therein."<sup>38</sup>

The picture was not uniformly bleak, however. Customary tenants on Crown estates apparently fared considerably better than their fellows on private estates, despite the adverse comment quoted above. In 1568 Elizabeth confirmed her Cumberland tenants on the payment of a two years' fine and in 1571 urged the local lords to follow her example.<sup>39</sup> In the various manors of the Richmond Fee, part of the barony of Kendal, fines in 16 Elizabeth were either one or two years' old rent, depending on the particular manor. A customary tenant on the Crown manor of Castle Sowerby paid a fine of one year's rent on the alienation of his holding, but no fine was paid by his heir after his death. The change of either lord or tenant called for a one-year fine in the manor of Holme Cultram, and each customary tenant also paid every five years a fixed fine—called a "running gressum"—which amounted to approximately three times the rent.<sup>40</sup> These examples are drawn from Crown estates, but certain lords also made no effort to push up fines. The Percys may have been easy toward their tenants, at least until the ninth earl of Northumberland began to improve his estates in the 1590s.<sup>41</sup> If the evidence can be trusted, the Dacres asked fines of but two or three years' rent on their extensive holdings—the baronies of Burgh, Gilsland, and Greystoke—even though the fines were arbitrary.<sup>42</sup> The Dacres may have been the last of the old breed of great northern magnates who saw their tenants as armed retainers rather than as mere entries on a rent roll. There is even a hint that the Dacres explicitly demanded tenant loyalty in return for low fines.<sup>43</sup> The Dacre lands reverted to the Crown in 1570, but fines remained low until the estates were returned to private hands.

As these examples show, it is possible to offer evidence to indicate that fines were reasonable—if one looks primarily at Crown estates—or exorbitant—if one looks at complaints about private landlords. Tawney was mistaken in asserting that the lords could raise fines as freely as they wished; the equity courts—supported by the Crown—stood in their way. On the

<sup>38</sup> Letter to Burghley, British Museum, Cotton MSS, Titus, F13, fols. 255v–56. See also reports from the Wardens of the Marches and Privy Council memorandums in Joseph Bain, ed., *The Border Papers: Calendar of Letters and Papers Relating to the Borders of England and Scotland* (Edinburgh, 1894–96), 1: 23–30, 106; 2: 131.

<sup>39</sup> Proposed act in Parliament, in Lemon and Green, *Calendar of State Papers, Domestic, Addenda 1566–1579*, 347–48.

<sup>40</sup> Crown estates rentals and surveys, PRO, L.R. 2/212, fols. 481v, 202, 308.

<sup>41</sup> G. R. Batho, "The Finances of an Elizabethan Nobleman: Henry Percy, Ninth Earl of Northumberland (1564–1632)," *Economic History Review*, 2d ser., 9 (1956–57): 433–50. See also the brilliant essay by M. E. James, "The Concept of Order and the Northern Rising, 1569," *Past and Present*, no. 60 (1973): 49–83.

<sup>42</sup> Survey, lands of Leonard Dacre, CRO, D/Howard of Greystoke Papers, 19, fol. 23v; and tenant depositions in PRO, E. 134/7–8 Chas I/H1.

<sup>43</sup> Statement of the benefits that would result from confirming the customs of the inhabitants of Cumberland, in Lemon and Green, *Calendar of State Papers, Domestic, Addenda 1566–1579*, 348.

other hand, Kerridge stresses that fines were “reasonable and modest, perhaps four or five years’ rent.”<sup>44</sup> The multiples he mentions seem to be low, at least for non-Crown properties. The Privy Council had proposed a statutory ceiling of no more than six years’ old rent for a fine as early as 1571.<sup>45</sup> It is unlikely that this figure would have been given unless it was often exceeded. In 1598 Chancery determined that an entry fine of ten years’ old rent was equitable.<sup>46</sup> The problem lies, however, in the definition of “reasonable.” Taken as a legal term, it was, of course, whatever a court said it was. Taken as an equitable term, indicating a fair division of the profits between a lord and his tenant, it is largely meaningless. We do not know enough about the average size of holdings, crop yields, land exhaustion, or markets—in short about the profit a “typical” tenant might enjoy—to be able to say whether two years’ or ten years’ old rent was “reasonable and modest.”

Whatever the tenants’ true condition on Elizabeth’s death, the union of the Crowns in 1603 clearly undermined the tenants’ legal protection. Their defense against unreasonable fines had been tenant right—the obligation to serve on the borders—and it had proved a useful plea in the distant equity courts. Now even this slender protection disappeared, for the borders no longer needed defending. The lords soon moved to nullify customary tenure and to force their tenants to take leases or else pay a lump sum for the confirmation of their customary estates and holdings. The Crown, in a reversal of Elizabeth’s moderate attitude toward her tenants in the north-west, made the first important move against the tenants. In the Court of Exchequer on June 15, 1610, tenant-right tenures in the Cumberland manors of Dacre, Blackhall, Lazonby, Staffield, Glassonby, Ainstaple, Irthington, and West Farlam were set aside and forty-year leases substituted.<sup>47</sup> The following year Lord William Howard, who had become lord of Gilsland in the right of his wife, a Dacre, moved against his tenants. Noting that previous Chancery decrees<sup>48</sup> had voided tenant right as a defense in equity, Howard “offered to make unto [his Gilsland tenants] estates for lives by warrant . . . or some other goode estates in lawe beinge unto them much better for their profitts.” At least this was Howard’s description of his offer. His tenants saw it in a different light; they believed that Howard “intended to extirpate them their Wives and Children out of their Tenements . . . and

<sup>44</sup> Tawney, *Agrarian Problem*, 287–310; Kerridge, *Agrarian Problems*, 43–44.

<sup>45</sup> Proposed act in Parliament, in Lemon and Green, *Calendar of State Papers, Domestic, Addenda 1566–1579*, 347–48.

<sup>46</sup> Equity proceedings, PRO. C. 3/284/34.

<sup>47</sup> Estate accounts, Howard of Naworth Papers, C176a.5. These leases appear to have been really a method of extracting lump-sum payments from the tenants to confirm for forty years their ancient customs. Under the confirmed customs they continued to pay the old rents, fines, and so forth. See *ibid.*, C176a.6. For an account of similar moves by landlords in Northumberland, see S. J. Watts, “Tenant-Right in Early Seventeenth Century Northumberland,” *Northern History*, 6 (1971): 72–74, 83–85.

<sup>48</sup> Now apparently lost, but referred to in Northumberland County History Committee, *Northumberland County History* (Newcastle-upon-Tyne, 1893–1940), 8: 238.

to plant strangers in their Roomes . . . and their Children should begg without releife or succor." The upshot of the dispute was a mass meeting of the tenants, who considered opposing Howard by force, followed by a series of cases in various equity courts.<sup>49</sup> Eventually Howard gave up trying to force the tenants to accept copyholds for lives and turned instead to demanding greater fines.

On the Percy estates the customary tenants were pressured to become leaseholders at double the old rent. They refused, evidently feeling that their right to inherit was defensible at law.<sup>50</sup> Seeking an alternative way to increase his revenues, the earl of Northumberland in 1616 wrote his representative in the Honour of Cockermouth, Sir Wilfrid Lawson, and complained that the entry fines of twenty years' old rent that Lawson was demanding of the tenants were not large enough. The earl's letter has not survived, and we do not know what he thought the tenants should pay. In his answer Lawson defended twenty-year fines, noting that they were "more than double so much as your lordship hath formerlie taken, if the bookes of fines be looked into."<sup>51</sup> The correspondence stops here and does not tell us if the earl continued to press for higher fines or was content with twenty years' old rent. The letter shows clearly, however, that entry fines on the Percy lands took a sharp jump upward in the years after 1603.

In 1619 Charles, the Prince of Wales, successfully proved that his tenants in the extensive barony of Kendal had no customary inheritable estates. In return for a payment of £2,700, Charles agreed to confirm his tenants' estates of inheritance, fix fines at two years' rent on the death of the lord and three years' on the change of tenant, and otherwise confirm the rights of the tenants. They agreed and thus obtained a satisfactory, if expensive, settlement.<sup>52</sup>

While this compromise was being thrashed out, King James issued a proclamation against tenant right. Published in 1620, it stated,

Whereas it hath beene ofentimes, by Decrees and Judgements at Law declared and settled, That Tenant-rights, since the most happy Union of these two renowned Kingdomes . . . are utterly . . . extinguished and abolished . . . And yet nevertheless divers Suits are continually raised and prosecuted . . . grounded upon the said claime of Tenant-right, or Customarie estate of Inheritance, under that pretence, whereby . . . both parties doe sustaine needlesse charge, and impoverishment . . . [and] may also . . . open a way to turbulent and seditious attempts: Wee . . . have both recommended . . . to all Our Judges, to surpresse and surcease strifes and suits of this nature, And have [ordered our officers to] let

<sup>49</sup> Complaint and answer, PRO, Sta. Cha. 8/161/16; abstract of proceedings, CRO, Mounsey-Heysam Papers, 6, pp. 2-8; H. S. Reinmuth, Jr., "The Struggle over Corby, 1605-1626: Lord William Howard and Thomas Salkeld," *Transactions of the Cumberland and Westmorland Antiquarian and Archaeological Society*, n.s. 66 (1966): 190-200.

<sup>50</sup> Wilfrid Lawson to earl of Northumberland, June 3, 1616, CRO, D/Lconfield Papers, Correspondence/169/1616.

<sup>51</sup> Wilfrid Lawson to earl of Northumberland, Oct. 28, 1616, *ibid.*

<sup>52</sup> Copy of decree in Chancery in PRO, State Papers Domestic, James I (hereafter S.P. 14), 203/11.

all Estates, whether for Lives or Yeeres, be it for Fine or improvement of Rent, by Indenture onely.

The proclamation then asked all the lords of the counties to follow the Crown's example by leasing their lands and commanded that there be no further mention of tenant right or customary estate for border service. In conclusion the proclamation expressed the hope that "good and dutifull" tenants would be treated considerably by the lords. If they were not, the equity courts were to "over-rule" the lords.<sup>53</sup>

Despite the proviso that the equity courts would find against any landlord who was excessively hard on his tenants, the proclamation probably appeared to many landlords as license to raise rents, convert to leases, or whatever they wished, and to evict those tenants unable or unwilling to meet their terms. Certainly this is what the tenants expected. Within a few months of the proclamation tenants of non-Crown manors in the barony of Kendal assembled to raise money for a common defense against their lords. The tenants sought to petition either the king or Parliament, asking that the lords be directed to confirm their customary tenure and fix their fines and obligations in return for some reasonable payment, as Prince Charles had done. The lords claimed that the assemblies were riotous, seditious, and in opposition to the king's proclamation, and the Crown hailed the tenants before Star Chamber.<sup>54</sup>

By bringing the tenants before Star Chamber the lords apparently wished to show them in the worst possible light and to crush once and for all any further opposition to leaseholds or arbitrary fines. But the tenants—or their counselors—shrewdly argued, first, that although they had been obligated to serve on the borders, they had been admitted as tenants in the various manors by the custom of the manor, paying such rents and fines as the manorial custom dictated. And nowhere did the pre-1603 manor court rolls say that they held by border service. In short, they claimed they held by double tenure: border service and custom. But by custom they were entitled to their estates. It is interesting to recall that prior to 1603 the tenants had argued just the opposite; then their other tenure, based on border service, was used to protect them from excessive fines. At that time the lords, not wishing to emphasize border service, had omitted it from their admittance rolls. Second, the tenants argued that if they had forfeited all rights to their holdings by the termination of border service, had not the lords forfeited their estates as well? The lords, too, had been obligated for border service; should not their lands return to the Crown?

James himself pressed the court to resolve the question of the tenants'

<sup>53</sup> James I, proclamation dated July 28, 1620, in J. F. Larkin and P. L. Hughes, eds., *Stuart Royal Proclamations*, vol. 1: *Royal Proclamations of King James I, 1603-1625* (Oxford, 1973), 488-90; also in PRO, S.P. 14/187, fol. 203.

<sup>54</sup> Any one of these offenses could bring the tenants before Star Chamber. This assembly was apparently directed only against the lesser lords, not against Prince Charles. For accounts of the affair, see proceedings, PRO, Sta. Cha. 8/34/4.

titles,<sup>55</sup> and Star Chamber accordingly named a panel of judges to “consider of the title and claim of both sides . . . that there may be absolute peace.” In 1625 the panel made its recommendations. The disputes in all the manors within the barony of Kendal were the same, the judges said, and were reducible to two: first, did the customary tenants have an inheritable estate, and, second, were fines fixed by custom or arbitrary at the will of the lord? The judges found for the tenants on the first; their estates were essentially copyholds of inheritance, even though they lacked “divers formalities” found in copyhold of inheritance in the south. The judges also found that border service “was no special part of their services” but a duty demanded not only of them but of all freeholders and lords as well. Nor was there “ever any mention of their border service in their admittances.” On the second point of dispute, the nature of the fines, the panel declined to offer an opinion, saying that this would exceed the instructions given them. However they strongly urged Star Chamber to adjudicate the matter. If left unresolved, they feared, disputes over fines would cause “endless suits.”<sup>56</sup>

It is not known if Star Chamber ever settled the problem of fines. Probably it did not, for litigation continued. In fact the two questions—of the right of inheritance and the size of the entry fine—are inseparable. As Tawney said, if the heir could not pay the fine, he effectively lost his father’s land, whatever the legal niceties.<sup>57</sup>

Certainly pressure for heavier fines continued after 1625, though no outright effort was made to deny the tenants their inheritance rights and convert them to leaseholders. We have already noted the increase on the Percy manors; many other lords assessed fines of twenty to thirty years’ rent.<sup>58</sup> On some manors “running” fines were imposed as an additional burden. The tenants of Hensingham, for example, every seven years paid a running fine of seven years’ rent, or what amounted to a doubling of the old rent. They also paid entry fines of from eighteen to thirty years’ rent.<sup>59</sup> In at least one case an Elizabethan court decision fixing fines at four years’ old rent was set aside, and the tenants were ordered to pay arbitrary fines.<sup>60</sup> By the end of the century fines as high as ninety-six times the old rent were demanded in the manor of Leversdale in the barony of Gilsland, although most fines there ranged from twenty to thirty years’ rent.<sup>61</sup>

<sup>55</sup> James’s instructions to the judges in the Court of Star Chamber, PRO, S.P. 14/154, fols. 11–12.

<sup>56</sup> The above quotes are from the proceedings of Star Chamber, printed in Joseph Nicolson and Richard Burn, *The History and Antiquities of the Counties of Westmorland and Cumberland* (London, 1777), 1: 56–59.

<sup>57</sup> Tawney, *Agrarian Problem*, 297–98.

<sup>58</sup> Numerous examples are given in the depositions in PRO, E. 134/7 Car 1/M38. See also William Hutchinson, *History of the County of Cumberland* (Carlisle, 1794), 1: 98, 132, 153, 175, 181, 208, 212, 405, 511, 577.

<sup>59</sup> Equity proceedings, PRO, E. 134/8 Chas 1/M16.

<sup>60</sup> On the manor of Calder. Deposition of Anthony Curwen, PRO, E. 134/8 Chas 1/M16.

<sup>61</sup> Estate accounts and rentals, Howard of Naworth Papers, C173.8.



On Crown manors fines tended to remain modest, perhaps because the tenants found the Crown willing to agree to low, certain fines if a lump sum was paid, in a manner similar to the settlement on the Kendal lands.<sup>62</sup> Some private lords followed the Crown precedent, confirming estates and fixing fines in return for a stipulated payment. In 1635, for instance, the tenants at Threlkeld paid Sir John Lowther £1,350 to confirm their customs and establish the fine of four years' old rent. This agreement, like others drawn at the time, called for a decree in Chancery and a private bill in Parliament to register the terms of the contract.<sup>63</sup> Toward the end of the seventeenth century, tenants in a growing number of manors compounded with their lords to make fines certain or even to convert their lands to freehold.<sup>64</sup> Economically these agreements were of long-term benefit to the tenant, although it may have been a considerable short-term strain to raise the necessary cash.<sup>65</sup>

The trend toward fixed obligations, however, was incomplete as late as the end of the eighteenth century. William Hutchinson, writing about Cumberland in 1794, deplored the "most numerous and strong remains of vassalage and servility retained in the customs of the manors within this county, that are to be found in any part of England." He noted that "the miserable tenant, who is going to pay an arbitrary fine and a heriot, is perpetually impoverished," adding that "those customary tenures are a national grievance. From this tenure is chiefly to be attributed the vast and dreary wastes that are found in Cumberland."<sup>66</sup>

A short summary of the evidence is in order here. It appears that landlords gradually increased the entry fines demanded of their tenants during the sixteenth century. Some lords were more grasping, some less, but the general trend was toward higher fines. The Crown, however, stood in the way of extreme exploitation, fearing that this would lead to a breakdown of border defense. The Crown also treated its own tenants with moderation. But after 1603 the Crown led the way in demanding greater payments from its tenants, although usually in the form of a single-settlement payment. Private landlords followed suit and demanded higher fines or the substitution of leasehold for customary tenure. The Star Chamber ruling in 1625 established that a customary holding was an estate of inheritance but left the problem of fines unresolved. And fines continued to creep up, in amount

<sup>62</sup> Rentals and surveys, PRO, Special Collections, Rentals and Surveys (S.C. 11), 1001, and PRO, Exchequer, Augmentation Office, Parliamentary Surveys (E. 317), Cumberland 1, 4, and 8. The settlement in the barony of Kendal has already been cited.

<sup>63</sup> Parish documents, CRO, Parish Chest/21/54 and 55. For another example, the manor of Caldbeck, see the agreement between the lords and tenants, CRO, D/Leconfield Papers, Caldbeck, box 219.

<sup>64</sup> For example, see the copies of agreements for Threlkeld in 1635 (CRO, Parish Chest/21/55), Bewcastle in 1630, Parton in 1672, Burgh by Sands in 1674, and Preston Richard in 1679 (Hutchinson, *History of the County of Cumberland*, 1: 79; 2: 461, 500; 1: 212).

<sup>65</sup> Usually these agreements were between some of the tenants and the lord. Other tenants could not, or would not, raise the money and remained subject to arbitrary fines.

<sup>66</sup> Hutchinson, *History of the County of Cumberland*, 1: 37-39, 133.

and diversity, unless the tenants were willing to make a contractual settlement with their lord that permanently fixed fines.

IF KERRIDGE CALLS FINES of four or five years' rent "reasonable and modest" for the last years of the sixteenth century, fines of twenty to thirty years' rent demanded a few years later, or fines of ninety-six years' rent toward the end of the seventeenth century, might fairly be termed "unreasonable." Prices did not rise proportionately.<sup>67</sup> It seems that after 1603 the lords were able to extract more from their tenants than formerly—and they did. The stark exploitation of the northwestern tenantry took place not in the sixteenth but in the seventeenth century, after the accession of James to the English throne.

Was this exploitation capitalistic, as Tawney would have it? I think not: it was rent exploitation, merely one of the tactics of the seigneurial reaction.<sup>68</sup> The lords exploited their tenants as sources of rent rather than as sources of power—as armed retainers—as they had in the fifteenth and early sixteenth centuries. But let us pursue the question: is rent exploitation capitalistic? Not unless one can say that the entire Western European "seigneurial reaction" was capitalistic. Rents—in the broad economic sense that includes fines, feudal dues, and labor services—were forced up almost everywhere, but an agricultural revolution took place in only a few favored regions, such as England. Rent increases are age-old; if they are not accompanied by investments in new techniques, improvements, and new crops they are unrelated to the transformation of agriculture that took place in England. Put simply, the change to commercial relationships that spelled the end of the older manorial ties between lord and tenant did not necessarily result in capitalism in any meaningful sense of the word. Capitalism only arose in certain areas where additional factors combined with this new commercialism to revolutionize the economy. Tenant-lord relationships in both the south and the north of England had been converted to a commercial, or cash, relationship. Beyond this similarity, the regions' economies differed greatly. Southern England had other conditions that led to capitalism; the northwest did not.

<sup>67</sup> The thirty-one year moving average price of wheat was 30.5 shillings per quarter in 1603, compared to 36 shillings per quarter in 1699, an increase of just under fifteen per cent. In the sixteenth century the price of wheat (also using thirty-one year moving average prices) rose from six shillings in 1500 to 29.8 shillings in 1600, approximately a fivefold increase, which seems to have exceeded the increase in rents or fines in the northwest. For prices, see W. G. Hoskins, "Harvest Fluctuations and English Economic History, 1480-1619," *Agricultural History Review*, 12 (1964): 28-46, and "Harvest Fluctuations and English Economic History, 1620-1759," *ibid.*, 16 (1968): 15-31. These prices are from the south and west; no suitable northern price series exists. The reader should be cautioned that northern oat price movements need not have closely paralleled those given here for wheat, particularly in the sixteenth century, before the development of a single market for all England. See the discussion of prices in Appleby, "Disease or Famine?" particularly pages 419-20.

<sup>68</sup> See Marc Bloch, *French Rural History: An Essay on Its Basic Characteristics*, tr. Janet Sondheimer (Berkeley, 1966), 126-49.

Kerridge has, I think, provided a key to understanding tenant-lord relationships in the northwest. "To assert that capitalism throve on unjust expropriations is a monstrous and malicious slander. Security of property and tenure answered capitalism's first and most heartfelt need. Where insecurity reigned, it was because of the absence, not the advent or presence of capitalism."<sup>69</sup> Is it unreasonable to see insecurity of property—that is, the possibility of loss either through eviction or ever-increasing fines—as one of the causes of the agricultural backwardness of the northwest? And is it unreasonable to see this insecurity as stemming from the absence of capitalism? In the northwest there was no attempt by the lords to enter into any partnership with the tenants, to share in the costs and profits of an agricultural revolution, that Kerridge finds elsewhere in England. And there was no agricultural revolution in Cumberland and Westmorland. The region remained an agricultural backwater well into the middle of the eighteenth century.<sup>70</sup> New crops were not introduced and new techniques remained unutilized, except on the demesne lands of a few lords. The northwest does not conform to the general English pattern but parallels that most commonly found on the Continent.<sup>71</sup>

This line of argument raises one other question: why was there no agrarian capitalism, as it has been defined here, in Cumberland and Westmorland? Why did the landlords follow a policy of rent exploitation? Precise answers to this twofold question are impossible, but it may have been that rent exploitation offered the best return that could be expected from such an isolated, largely upland region. Overland markets were distant, and before the middle of the seventeenth century seaports were almost totally undeveloped.<sup>72</sup> In addition the region had a limited agricultural potential because of poor soil and an inclement climate.<sup>73</sup> Improvements or innovations would have been of no value in the uplands and, without nearby markets to absorb the expanded production, of little value in the lowlands. The one useful change would have been improvement in the breed of sheep, but this could be undertaken by the occasional great sheep raiser without the cooperation of his tenants. Given these conditions, rent

<sup>69</sup> Kerridge, *Agrarian Problems*, 93.

<sup>70</sup> Kerridge, *Agricultural Revolution*, 277, 287.

<sup>71</sup> D. C. North and R. F. Thomas have suggested that changes in the manorial system brought about "fee-simple absolute ownership of land and a free market for labor." These, they note, are "two essential preconditions for efficient resource allocation and, ultimately, for economic growth." "The Rise and Fall of the Manorial System: A Theoretical Model," *Journal of Economic History*, 31 (1971): 777–803. I think it is clear that land ownership in the northwest was not absolute. A *de jure* free market for labor existed, but before alternative employment developed locally, I wonder if there was not a psychological predisposition to squat on the moor or in the forest that in fact limited the free market for labor.

<sup>72</sup> In 1633 Whitehaven had nine or ten cottages, according to Hutchinson, *History of the County of Cumberland*, 2: 49. By 1685, 268 families lived there. See the count of inhabitants in CRO, D/Lonsdale, Whitehaven, Papers, commonplace book beginning "Mr. Drydens . . .," p. 7.

<sup>73</sup> Agricultural descriptions of the region are found in Joan Thirsk, "The Farming Regions of England," in Thirsk, *Agrarian History*, 16–28; and Kerridge, *Agricultural Revolution*, 160–65, 171–73.

exploitation probably yielded more profit than any program of improvement would have.

There is even a certain consistency between enclosure and rent exploitation. As we have seen, enclosures took the form of encroachments on the common pasture, whether in upland moor or lowland forest, and encroachments returned rent. This was not the only possible use that could have been made of the land. For example, the earl of Northumberland could have, had he wished, converted the Forest of Westward to large-scale cattle raising. But the distance of Westward from the major consuming markets far to the south would have put him under a competitive disadvantage. Perhaps for this reason he opted to open it to enclosure—to rent it out. I do not mean to say that the earl carefully weighed these alternatives; they may not have occurred to him. Renting the forest was a perfectly rational choice, however, considering the geography and isolation of the region.

The reader might confidently expect that the condition of the customary tenants, squeezed dry by fines, and the miserable squatters, subsisting on their plots of ground, steadily worsened throughout the seventeenth century. This was not the case. Apparently rural population stopped increasing soon after 1600, and pressures on the remaining pasture land eased. The rate of encroachment fell off sharply and remained at a low level until 1700.<sup>74</sup> Coal, iron, and lead mining had been carried on in a limited way in the sixteenth century, and by 1650 mining had expanded to the point where it provided by-employment for many smallholders. At about the same time trade with Ireland began to flourish and prompted a rapid growth in shipping along the west coast of Cumberland. The number of customary tenants declined,<sup>75</sup> no doubt in part because a tenant could, if the lord demanded an excessive fine, now walk away from his customary holding and still find a living within the region. In the northwest the economy was transformed, but it was transformed by an expansion of trade and industry, not through a revolution in agriculture.

<sup>74</sup> Dilley, "Common Land in Cumberland," 93–95.

<sup>75</sup> G. P. Jones, "The Decline of the Yeomanry in the Lake Counties," *Transactions of the Cumberland and Westmorland Antiquarian and Archaeological Society*, n.s. 62 (1962): 211–12.

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## Native American History: How It Illuminates Our Past

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A Review Article by WILBUR R. JACOBS

WILLIAM BRANDON. *The Last Americans: The Indians in American Culture*. New York: McGraw-Hill Book Company. 1974. Pp. v, 533. \$12.95.

JOSEPH G. JORGENSEN. *The Sun Dance Religion: Power for the Powerless*. Chicago: University of Chicago Press. 1972. Pp. x, 360. \$20.00.

ELEANOR BURKE LEACOCK and NANCY OESTREICH LURIE, editors. *North American Indians in Historical Perspective*. New York: Random House. 1971. Pp. xi, 498. \$14.00.

ELÉMIRE ZOLLA. *The Writer and the Shaman: A Morphology of the American Indian*. Translated by RAYMOND ROSENTHAL. New York: Harcourt Brace Jovanovich. 1973. Pp. viii, 312. \$12.50.

C. A. WESLAGER. *The Delaware Indians: A History*. New Brunswick: Rutgers University Press. 1972. Pp. xix, 546. \$17.50.

BERNARD W. SHEEHAN. *Seeds of Extinction: Jeffersonian Philanthropy and the American Indian*. Chapel Hill: University of North Carolina Press, for the Institute of Early American History and Culture. 1973. Pp. xii, 301. \$11.95.

CECIL EBY. "*That Disgraceful Affair,*" the *Black Hawk War*. New York: W. W. Norton and Company. 1973. Pp. 354. \$9.95.

LAWRENCE R. MURPHY. *Frontier Crusader—William F. M. Army*. Tucson: University of Arizona Press. 1972. Pp. xii, 313. Cloth \$9.50, paper \$5.95.

CHARLES J. KAPPLER, compiled and edited by. *Indian Treaties, 1778-1883*. With a new foreword by BRANTLEY BLUE. (A Sol Lewis Book.) Reprint; New York: Interland Publishing. 1972. Pp. 1099. \$67.50.

WILCOMB E. WASHBURN. *The American Indian and the United States: A Documentary History*. In four volumes. New York: Random House. 1973. Pp. xiv, 714; vi, 715-1444; vi, 1445-2266; viii, 2267-3119. \$110.00 the set.

IN REVIEWING THE LATEST historical literature, we often find extraordinary flashes of interpretation illuminating revised editions of our best short histories. A sign of the times can be seen in Ray A. Billington's fourth

For criticism of this essay, I am grateful to my colleagues at the University of California, Santa Barbara, Alexander DeConde, C. Warren Hollister, and Richard Oglesby, and to William Swagerty, a doctoral candidate.

edition of *Westward Expansion*, which contains a basic shift of interpretation reflecting much of the recent scholarship in native American history. In reappraising "the Indian in the expansionist process," Billington argues that the Indian's "major role" makes the change necessary. His new chapter on the woodland peoples, for example, is a very convincing portrayal of Indians as a people with a unique culture who were exploited by the advance of the white frontier. What Billington has given us in this vividly written chapter is a fresh analysis of white frontier history by looking at the past from the perspective of native Americans. Here, in what many consider to be the most influential one-volume history of the West, we have, at last, recognition that the Indian has a history worth studying in itself. And the reason, of course, is that native American history is closely intertwined with, if not inseparable from, the general history of the United States. Even the editors of the New American Nation series have decided to include a volume, *The Indian in America* by Wilcomb E. Washburn, which at last gives us an expanded portrait of a neglected part of the American past. With a series of splendid chapters based upon interdisciplinary sources and monographs, Washburn writes at length about such topics as the Indian personality, Indian social structure and wars, coercion of the Plains tribes, and native American identity. Herein, indeed, is a fresh view of Indian-white relations.<sup>1</sup>

Much of the same kind of perspective given us by Billington and Washburn is found in the brilliant but lengthy legal briefs on the Sioux Treaty of 1868, prepared by lawyers John Thorne and Larry Leventhal for the 1974 Wounded Knee trials held in St. Paul, Minnesota, and in Lincoln, Nebraska.<sup>2</sup> In mobilizing its defense, the legal team of the American Indian Movement dipped into the ethnohistory of Sioux treaties to show them as part of "the supreme law of the land." Again and again the relevant Indian oral history added a new dimension to legal deliberations. In the same way, the Indian Historical Society in San Francisco, through its able editor Jeanette Henry and through such publications as *Textbooks and the American Indian* and the journal *Indian Historian*, has exposed much of the claptrap

<sup>1</sup> Ray A. Billington, *Westward Expansion: A History of the American Frontier* (4th ed. rev.; New York, 1974), viii, 15-28; Wilcomb E. Washburn, *The Indian in America* (New York, 1975), 11-65, 126-208. Robert Trennert, "The Far Western Indian Frontier and the Beginnings of the Reservation System, 1846-1851" (Ph.D. dissertation, University of California, Santa Barbara, 1969), is consistently cited by Washburn in his unique account of the dispossession of Plains tribes (pp. 171-94). Trennert's valuable manuscript is soon to be published by the Temple University Press under the title "Alternative to Extinction."

<sup>2</sup> Larry Leventhal, "United States District Court, District of South Dakota, Western Division, United States of America Plaintiff, vs Dennis Banks, Russell Means, Defendants, *The Sioux Treaty of 1868: Memorandum*," mimeographed [St. Paul, 1973]; John E. Thorne, "United States District Court, District of South Dakota, Western Division, Sitting in the District of Nebraska . . . Motion to Dismiss for Lack of Jurisdiction under the Sioux Nation—United States Treaty of 1868 (Fort Laramie Treaty)," mimeographed [Lincoln, 1974]. Judge Warren K. Urbom's "Memorandum and Order on Motion to Dismiss for Want of Jurisdiction, Consolidated Wounded Knee Cases," Jan. 14, 1975, typescript [Lincoln], is also a significant document. See also Martin Waldron, "Indians Ask U.S. to Declare Sioux a Sovereign Tribe," *New York Times*, Dec. 29, 1974, p. 29.



and racism that have long dominated Anglo-American Indian policy and historical writing.<sup>3</sup> Expansion of such themes in the field of Indian journalism is found in the excellent newspaper *Wassaja*, published by the Indian Historical Society, and the journal of the Mohawk Nation, *Akwesasne Notes*. Both of these newspapers have tied the present to the past in an effort to mobilize Indian resistance against further coercion by whites.<sup>4</sup>

Themes of native American history are thus appearing in newspapers, historical magazines, and books and are used to justify outright physical resistance to white authorities at Pine Ridge, Custer, and elsewhere by stressing U.S. government misconduct as a historic phenomenon that Indian people have had to endure.<sup>5</sup> Moreover in recent years talented Indian writers such as Vine Deloria, Jr. and N. Scott Momaday have made the literate American citizenry more aware of the special significance of our Indian heritage. While one must immerse himself in the characters in Momaday's *House Made of Dawn* to see concepts of ecological balance and order in the native American conception of the universe, Vine Deloria, with irony, good humor, wisdom, and erudition, makes us listen to his plea that *Custer Died for Your Sins*, that *God Is Red*, or that there is a sordid tale *Behind the Trail of Broken Treaties*. Deloria's editing of *Treaties and Agreements . . . of the Sioux Nation*, published under the imprint of the new Institute for the Development of Indian Law, gives us material from representative manuscripts that has been overlooked. And his forthcoming book-length study on the Sioux Treaty of 1868 will give Americans the first analysis of a major Indian treaty made by a member of the legal profession who is himself Indian. Of equal significance is the persuasive new volume by Kirke Kickingbird (Kiowa) and Karen Ducheneaux (Sioux), *One Hundred Million Acres*, a powerful historical manifesto detailing native American land losses.<sup>6</sup>

<sup>3</sup> Jeannette Henry, *Textbooks and the American Indian*, ed. Rupert Costo (San Francisco, 1970) evaluates several college-level volumes but places special emphasis upon secondary textbooks. Jeannette Henry is also editor of the series *Educational Perspectives*, published in San Francisco by the Indian Historical Society, which includes *The American Indian Reader: Anthropology* (1972), *The American Indian Reader: Education* (1972), *The American Indian Reader: Literature* (1973), and *The American Indian Reader: History* (1974).

<sup>4</sup> See, for example, the April-May 1974 issue of *Wassaja* and the April 1974 issue of *Akwesasne Notes* for articles on historical topics bearing on the Wounded Knee trials, policies of the Bureau of Indian Affairs, and misrepresentation of native Americans by museums.

<sup>5</sup> An excellent illustration of this kind of journalism is found in Vine Deloria's learned analysis, "The Question of the 1868 Sioux Treaty . . . A Crucial Element in the Wounded Knee Trials," *Akwesasne Notes*, Apr. 1974, p. 12.

<sup>6</sup> See N. Scott Momaday, *The House Made of Dawn* (New York, 1969), and his penetrating review essay, "Bringing On the Indians," *New York Review of Books*, Apr. 8, 1971, pp. 39-42. Also see Vine Deloria, Jr., *Custer Died for Your Sins: An Indian Manifesto* (New York, 1969); *God Is Red: A Study of Christianity and the American Indian* (New York, 1973); and *Behind the Trail of Broken Treaties: An Indian Declaration of Independence* (New York, 1974). Deloria's edition of *Treaties and Agreements and the Proceedings of the Treaties and Agreements of the Tribes and Bands of the Sioux Nation* [Washington, 1973] includes archival materials heretofore unpublished. It is one of eight volumes of the American Indian Treaty series, compiled by geographic region. The series, published by the Institute for the Development of Indian Law, Washington, D.C., also includes a chronological list of treaties with an introduction by Deloria. Finally, see Kirke Kickingbird and Karen Ducheneaux, *One Hundred Million Acres* (New York, 1973).

With such accomplishments by Indian writers, what have Anglos<sup>7</sup> contributed? As noted above, Billington's revised text is significant, probably because it is on the crest of a literal avalanche of books and articles by non-Indians. Many of these are by ethnohistorians who know their way in and out of interdisciplinary and comparative studies. While it is true that Indians often question the value of native American history written by whites because of persistent threads of ethnocentrism in the fabric of white scholarship, it is also true that the white contribution toward understanding the Indian heritage should not be underestimated. William N. Fenton, perhaps our leading Iroquois scholar, argues that it is "a genetic fallacy" to exclude whites from native American studies. Besides, the great anthropologist Lewis H. Morgan was a WASP.<sup>8</sup>

Certainly Fenton's point is well taken. And even the Indian defense team in St. Paul and in Lincoln was willing to have Anglo historical witnesses on their side, such as barbershop Indians W. R. Jacobs, Alvin Josephy, and Dee Brown, to buttress the expert testimony given by Vine Deloria, Roger Buffalohead, and Bea Medicine. We should acknowledge, however, that Indians often have their own version of what happened in the past. Perhaps we can benefit by a synthesis of both white and native American ethnohistory.

A Flathead native-American writer who inspires all of us by his appreciation of the coercion endured by Indian people is D'Arcy McNickle. He was among the first of the scholarly world to give respectability to the idea that confederation tribes of the woodlands may have had an influence on the formation of the Constitution, a theme that has attracted William Brandon, one of the most learned and skilled of the modern Anglo writers on what can be called the new Indian history.

Brandon's *Last Americans* is a revision, with the addition of copious bibliographical annotations, of *The American Heritage Book of Indians*, a volume distinguished by its magnificent illustrations.<sup>9</sup> It sometimes takes several rewritings and editions to bring a superb book to fruition, but here we have it: a work many will judge to be the finest one-volume history of the American Indian. Brandon's book has narrative eloquence, blended with profundity of generalization, shrewd realism, moral dignity, sly humor, irony, and historic truth. The truth is there because it emerges from decades

<sup>7</sup> "Anglo" is a general name used in the West, especially on reservations, for non-Spanish whites.

<sup>8</sup> In his commentary given at the session on "American Indian History and Historiography," April 28, 1974, annual meeting of the Organization of American Historians in Denver, Colorado, Fenton argued: "*There is no special gene for culture; it is learned.* The outside observer who is trained can often isolate the patterns that guide behavior that seldom if ever rise to a conscious level among participants. Advocates of ethnic chauvinism [stating] that Indian studies are best learned and taught by Indians to Indians may not be aware that in the extreme they are advocating the kind of racism that beset learned studies, particularly the social evolutionists, of the nineteenth century, a view from which Boas delivered anthropology in 1911."

<sup>9</sup> William Brandon, *The American Heritage Book of Indians* (New York, 1961). The text was published separately in paperback by Dell Publishers (New York, 1964).

of study of the ethnological, archeological, historical, artistic, and literary sources. Here in one volume is a historical portrait of native Americans, embracing the entire Western Hemisphere from prehistory to modern times.

Yet Brandon's emphasis is on the history of Indian-white relation in the United States. It is here, for instance, that we turn for a masterful summary of what happened in the removal era and in the later dramatic confrontation between the Plains Indians and the U.S. Army. Brandon's annotations, almost as much fun to read as the lively text, wipe away the cobwebs spun by other writers and help us to see, for instance, Andrew Jackson's key role in implementing an anti-Indian policy. As Brandon understands the Indian perspective, so he comprehends the white attitudes in such comments as "indolence is probably the most used single word in all reports written by non-Indians about Indians to the present day." Native Americans, he contends with ample evidence to support his view, were more concerned with "living" than "getting." Perhaps the most effective literary device Brandon uses is to let his leading characters talk for themselves, as, for instance, when he allows that reluctant Indian fighter, General George Crook, to speak, "Greed and avarice on the part of the whites—in other words the almighty dollar, is at the bottom of nine-tenths of all our Indian troubles."<sup>10</sup>

In another volume of the new Indian history, the anthropologist Joseph G. Jorgensen's penetrating work, *The Sun Dance Religion: Power for the Powerless*, Crook's shrewd observation is borne out in a searching ethno-historical work. Jorgensen's case, convincingly presented in this carefully documented volume, is that in the late nineteenth century the Sun Dance was largely responsible for bringing about a powerful revitalization among the Shoshones and Utes, peoples long exploited by their white neighbors. The Sun Dance helped them endure and then cast off the evils of a white society that expropriated their resources and manipulated their labor. Always faced with the difficulty of finding a way of accommodating the ideals of Indian collectivism to Protestant individualism, Indian people were gradually forced into what Jorgensen calls "neocolonial niches" and victimized by a metropolitan economy. The agents of the whites were primarily the military, the Bureau of Indian Affairs, or small-time entrepreneurs—stock raisers, shopkeepers, and farmers. Jorgensen also contends that these whites manipulated tribal peoples into agribusiness corporations, which proved to be a most unsatisfactory form of collectivism for Indians.

Here is ethnohistory at its best, but with a bit more detail on the Sun Dance rituals than the average historian may want to read. On one point, however, the historian may chide Jorgensen. Although he offers evidence to show that the Sun Dance was an outgrowth of deprivations suffered by two major tribes at the hands of greedy whites, he does not really come to grips with the issue of whether or not the original Sun Dances had other

<sup>10</sup> Quoted in Brandon, *Last Americans*, 379.

functions and how exactly the transformation to the modern Sun Dance took place.<sup>11</sup> But this is a challenging historical question, and material evidence—that is, conclusive proof from Indian oral history and documentary sources—is probably impossible to find.

Jorgensen's book emphasizes that Indian people have great and powerful natural endowments in the world of the spirit as well as an instinct for self-preservation. These are viable resources that both Indians and modern Anglo scholars have not always appreciated. Yet the Sun Dance, the Ghost Dance, and the Peyote Cult have long been recognized as revitalization movements, though no writer has so thoroughly evaluated the Sun Dance phenomenon as Jorgensen.

*North American Indians in Historical Perspective*, essays by anthropologists edited by Eleanor Leacock and Nancy O. Lurie, frequently spotlights Indian prophets and their significance as leaders of spiritual movements. Probably the most valuable essay in this volume is William N. Fenton's penetrating "The Iroquois in History," which shows the Six Nations as a people who relied on their prophets to periodize their history. Thus, as Fenton outlines Iroquois history, a formative period begins with the founding of the Longhouse by Daganawidah and the most recent era is the time span since the prophecy of Handsome Lake. Fenton speaks Mohawk and has been adopted into a tribe, and like Lewis H. Morgan, Frank Speck, and other leading Anglo investigators of our Indian heritage, he has deep feelings and affection for the people about whom he writes. Thus he is able to bring to modern scholarship an appreciation of the admirable oral tradition of the Iroquois in which sachems with verbatim recall dispute in learned arguments traditional but conflicting versions of Iroquois history.<sup>12</sup>

Other writers in this volume include D'Arcy McNickle and the late Edward Dozier, both of Indian heritage and respected scholars. Their contributions are of special significance since they express an Indian perspective. McNickle's sweeping overview, "Americans Called Indians," and Dozier's survey of the Southwestern tribes are ethnohistorical social portraits that are a delight to read. Moreover they are a silent testimony to the fact that we need more of the new Indian history. When McNickle argues that the Pueblo tribes have been under the threat of dissolution or extermination longer than other Indians of North America, he makes us understand that among these people conformity is a way of life, a price to pay for survival in a hostile world of outside threats. William C. Sturtevant's perceptive essay in this collection shows how white pressures may have been so great that Indian groups actually emerged from the metamorphosis as a

<sup>11</sup> Jorgensen, *Sun Dance Religion*, 17–24, 26–85.

<sup>12</sup> Another example of Fenton's extraordinary grasp of Indian studies is his lucid article on the Iroquois wampum-belt controversy, "The Case for the Integrity of Cultural Treasures," *Proceedings of the American Philosophical Society*, 115 (Philadelphia, 1971): 430–71. Fenton's views are contested, however, in a series of resolutions and articles in the *Indian Historian*, 1970, no. 1, pp. 4–18, 50.

transformed people, such as the Seminoles, a tribe of post-European origin.

Almost without exception the essays in this fine volume, originally given as papers at a European conference, are worth reading. The weakest, an overall survey of American Indian history by Nancy Lurie, has insights, though it is written with the flippancy of overconfidence. Her work is also flawed by lack of investigation into historical sources and monographs.

AN INCISIVE BOOK-LENGTH ASSESSMENT of white attitudes toward native Americans is found in Elémire Zolla's *Writer and the Shaman*. For an Italian specializing in American literature at the University of Genoa who depends entirely upon published sources, this author has given us a remarkably clear view of how the idea of progress permeated American thought and was sometimes used as a justification for extermination of tribal peoples. Mincing no words, Zolla makes his case:

Thus the idea of progress has not only justified and promoted slaughter—at times physical, at times spiritual, depending on the circumstances—but it has even removed it from the consciousness. Indeed, when it was opportune, the idea of progress also remolded, revised the Indian's image, and even made it indivisible.<sup>13</sup>

As Zolla builds his argument, he explains that key figures, such as the Puritan missionary John Eliot, writing when European attitudes toward the Indian were developing, set forth maxims in Indian-white relations. Eliot, for instance, somehow convinced generations of Americans that the Indian had to become civilized before he could be converted. In other words, deculturalization had to take place before acculturation, a line of thinking adopted by Catholic missionaries as well as Puritans. In tracing the imagery conjured up by a whole panoply of writers following Eliot—Benjamin Franklin, George B. Grinnell, Willa Cather, Edmund Wilson, and N. Scott Momaday among others—Zolla finds relatively few who rose above this arrogance toward the Indian. However true this theme may be in American literary annals, Zolla's reliance on it is, I think, confining. For instance, his discussion of Francis Parkman's work is limited entirely to Parkman's youthful volume of 1846, *The Oregon Trail*, although Parkman had much to say about Indians after that.<sup>14</sup> This kind of surveying of writers, giving each one a puff of appraisal, is characteristic of literary histories, but such a small sampling of an author's work is particularly hazardous. Zolla is well read, however, and partly relies on other writers—Henry Adams's history of the presidencies of Jefferson and Madison, for instance—in appraising Jefferson's hypocrisy in urging both progress—Indians should cultivate the soil—and a spirit of benevolence.

<sup>13</sup> Zolla, *Writer and the Shaman*, 3.

<sup>14</sup> See, for instance, an appraisal of Parkman's use of the conspiracy theory and its racist implications (as set forth in *The Conspiracy of Pontiac and the Indian War after the Conquest of Canada* [Boston, 1898] in W. R. Jacobs, *Dispossessing the American Indian: Indians and Whites on the Colonial Frontier* (New York, 1972), 83–93.

We may be reasonably sure that the theme of progress justifying Indian genocide is true enough from the many examples Zolla has given us and from what we already have from Winthrop Jordan, Roy Harvey Pearce, and Richard Slotkin. Slotkin, however, in his monumental study on the mythology of the frontier, shows us how profoundly complex the myth-theme of progress actually is and how it relates to concepts of property, purification of the land, and a host of other myth-themes.<sup>15</sup>

For a practical lesson in what actually happened as a result of white attitudes of progress we may turn to tribal histories. These histories, written by the Indians themselves, are indispensable, though some modern Indian historians have a certain disdain for the Anglo practice of footnoting factual data. As the Nez Perce contend, when they are telling the story of their own history and culture, there is no need to prove it by annotations.<sup>16</sup>

One of the best recent tribal histories written by a non-Indian is Clinton Alfred Weslager's *Delaware Indians: A History*, the product of decades of study by a layman archeologist-historian. With a rich blend of archeology, anthropology, Indian oral traditions (he gives us one of the best accounts of the Walum Olum, the fascinating hieroglyphics depicting the tribal origins of the Delaware), and documentary research, Weslager writes for the general reader as well as the scholar. The value of this book is illustrated by the author's re-evaluation of the controversies about the authenticity of the Walum Olum as a source for understanding the Delaware life-style, a melding of pacifism and religion. In the best traditions of ethnohistory he explores the past to give Americans a better appreciation of the trauma suffered by a deeply religious, agricultural people. After reluctantly giving up pacifist ideals that had made them such congenial allies of William Penn, they survived war and dispossession under daring chiefs and wise prophets. Weslager portrays the famous Delaware leader of the war of 1756, Chief Teedyuscung, as a gullible peace negotiator who was bribed by expensive gifts and thus used as a pawn in a maneuver to justify the infamous Walking

<sup>15</sup> See Winthrop Jordan, *White over Black: American Attitudes toward the Negro, 1550-1812* (Chapel Hill, 1968), 12-14, 89-91, 162-63; Jordan, *The White Man's Burden: Historic Origins of Racism in the United States* (New York, 1974), 46-50; Roy H. Pearce, ed., *The Savages of America: A Study of the Indian and the Idea of Civilization* (rev. ed.; Baltimore, 1965), 56-75; Richard L. Slotkin, *Regeneration through Violence: The Mythology of the American Frontier, 1600-1850* (Middletown, 1973), 3, 294, 338, 342, 348-49, 355-56, 563-65. See also Gary Nash, *Red, White and Black: The Peoples of Early America* (Englewood Cliffs, 1974), 34-45.

<sup>16</sup> Volume 1 of *Noon Nee-Me-Poo (We, the Nez Percés): Culture and History of the Nez Percés*, by Allen P. Slickpoo, Sr., project director, with the technical advice of Deward E. Walker, Jr. of the University of Colorado, was published by the Nez Perce tribe of Idaho (1973). This volume, containing long excerpts from treaty negotiations, might well have included some acknowledgment of sources without compromising tribal dignity. However, the statement is made that documents cited are on file at tribal headquarters (p. 7). See page 83 for an example of a long quotation from tribal proceedings in 1855. The defensive tone of the Nez Perce history is illustrated in the prefatory statement, "We have no sympathy with those who have written derogatory books about us and have chosen to ignore most of them. They are not our friends" (p. viii). Another Nez Perce publication of importance is *Nu Mee Poom Tit Wah Tit (Nez Perce Legends)* also by Allen P. Slickpoo, Sr., Nez Perce tribe director, with illustrations by Leroy L. Seth and technical advice by Deward E. Walker, Jr. of the University of Colorado (Nez Perce tribe of Idaho, 1972).





Fig. 1. Tishcohan, one of the Delaware chiefs who approved the notorious Walking Purchase of 1737 in which the Indians bargained away a large portion of Delaware land. Portrait by Gustavus Hesselius in 1734. Courtesy Historical Society of Pennsylvania.

Purchase of 1737. Teedyuscung, it appears, was actually persuaded to withdraw his charges of forgery and wrongdoing against the Pennsylvania authorities. The argument is well documented and convincing.<sup>17</sup>

<sup>17</sup> In appendix 2 of his volume, Weslager also publishes a letter written by Henry Schoolcraft to E. G. Squier, February 16, 1849, which gives an astute appraisal of the authenticity of the Walum Olum; see also pages 234–36. Though not under review in this essay, Arrell M. Gibson's excellent one-volume history, *The Chickasaws* (Norman, 1971), ranks as one of the best tribal histories to appear in print, the product of interdisciplinary study. Still another scholar at home in the various disciplines necessary to write Indian history is Warren L. Cook. His *Flood Tide of Empire: Spain and the Pacific Northwest, 1543–1819* (New Haven, 1973) details the impact of Europe on the Northwestern coastal tribes. And a recent doctoral dissertation by Calvin L. Martin shows the impact of Europe's frontier on the Northeastern tribes of the Micmacs and

PRACTICALLY ALL NATIVE AMERICAN history after the coming of the white man is partly the history of Indian-white relations. Beyond this, of course, lie fields of investigation that only indirectly focus on Indians. Three recent books that fall into this classification are basically studies depicting phases of Anglo-American westward expansion: Bernard W. Sheehan's *Seeds of Extinction: Jeffersonian Philanthropy and the American Indian*; Cecil Eby's "*That Disgraceful Affair,*" *the Black Hawk War*; and Lawrence R. Murphy's *Frontier Crusader—William F. M. Arny*. Sheehan's volume is primarily intellectual history, a descriptive analysis of white attitudes toward Indians in the age of Jefferson, while Eby's study, a readable narrative of a key war in the removal years in the 1830s, fills a gap in our history, but it is largely an account of the frustration of whites in attempting to annihilate a small band of Sauk-Fox tribesmen. Murphy's biography faithfully traces the tracks of a frontier crusader, William Arny, who spent a major portion of his life trying to convert Southwestern tribes into God-fearing Christian farmers.

As these three books are by and about whites, they probably should be evaluated that way. Sheehan's heavily annotated monograph, a revised doctoral dissertation, is a thoroughgoing investigation into published sources which argues that Jeffersonian philanthropists misunderstood Indians, who were incapable of assimilation into the mainstream of American life. As Sheehan contends, "The philanthropic plan to transmit civilization through education faltered in face of the Indian's persistent habit of indulging in a level of ferocity inconsistent with the white man's notion of humane civilization." Though whites were capable of barbaric orgies in killing Indians, Sheehan argues that "the flexibility of civilization permitted the white man to maintain forms of civilized life and at the same time to act like a savage." Not unexpectedly he concludes that "separation of the Indian's person from tribal discipline was the essential step in the civilizing process."<sup>18</sup> As seen from these quotations, understanding the author's interpretations is sometimes difficult. We are not sure if he is arguing the viewpoint of the Jeffersonian philanthropists, their critics, and their contemporaries, or if he is actually attempting to elucidate statements of fact. Then, too, there will be readers who may be unconvinced by Sheehan's contention that Jeffersonian philanthropists were consistently motivated by benevolent ideals.

Chippewas: "Keepers of the Game: The Ecological Issue in Indian-White Relations" (Ph.D. dissertation, University of California, Santa Barbara, 1974). These show that in Indian studies, some historians are becoming anthropologists and vice versa. One of the greatest scholars of native American history, the late Sherburne Friend Cook, a physiologist, has given us a most valuable essay, "The Significance of Diseases in the Extinction of the New England Indians," *Human Biology*, 45 (1973): 485-508. For an evaluation of Cook's work in Indian demography and that of his talented disciple, Henry Dobyns, an ethnohistorian-anthropologist, see W. R. Jacobs, "The Tip of an Iceberg: Pre-Columbian Indian Demography and Some Implications for Revisionism," *William and Mary Quarterly*, 3d ser., 31 (1974): 123-32.

<sup>18</sup> Sheehan, *Seeds of Extinction*, 190, 205, 278.

Cecil Eby's volume on the Black Hawk War is weak in ethnological understanding of the Indian life-style. There is little appreciation of the cultural shock sustained by Indians who were crushed by the advancing frontier. Written by a specialist in English language and literature, the book is frustrating for the conventional historical scholar because of the lack of citations. It is further weakened because the author tends to accept at face value such early nineteenth-century writers as Samuel Drake.

Murphy's excellent book, originally a doctoral dissertation, clearly documents the role that William Army had in rashly attempting to thrust a nineteenth-century Protestant reform program on nearly everyone in sight, though the Indians were the ones who had to endure his self-righteous crusade to "civilize and Christianize." With no hesitation, Murphy recognizes the evils of the concept of progress that Zolla discerned in writers of early American literature. Sheehan and Eby also recognize a theme of progress, but neither seems aware of the dilemma of Indian people in the deculturation process.

Greater appreciation by scholars of the complexity of ethnicity is needed. It is no easy task to follow a conscious and systematic methodology for comprehending the cultural traits of various ethnic groups in historical situations. Yet, the alternative, as Gunnar Myrdal has written, is the growth of American romanticism in scholarship, which often prevents serious, intensive study of cultural traits and tends to serve conservative and even reactionary schools of thought.<sup>19</sup>

Whatever mode of writing historians use to study the Indian, they will probably never abandon traditional documentary evidence. If we turn to the kind of printed material now available, as, for instance, the text of treaties as found in the reprinted edition of Charles J. Kappler's second volume on Indian affairs, *Indian Treaties, 1778-1883*, we have a useful reference tool. Unfortunately there is no editing. Original marginal notes that Kappler copied from documents are reproduced, and the index, primarily an alphabetical list of tribes attending treaty meetings, remains as it was first printed in 1904. Here is an expensive reproduction distinguished only by several pages of unenlightened introductory commentary and a colored frontispiece of Plains Indians on the move.

If one is interested in finding out what really went on, he would find the study of Indian treaties to be almost as tricky as presidential tapes. A qualified editor of Kappler's treaties, for example, might have backtracked from the treaties into other government publications to give us a meaningful edition. For instance, in tracing the various chronological highlights of the birth, adolescence, and maturity of the treaty of 1865 between the Arapaho and Cheyenne and the U.S. government following the Sand

<sup>19</sup> Gunnar Myrdal, "The Case against Romantic Ethnicity," paper presented at the Conference on Ethnicity and Historical Consciousness or Identity in the Modern World, May 13-15, 1974, Santa Barbara, California.

Creek Massacre, we find that when originally presented to the Senate it contained a censure of Colonel Henry Chivington as a result of the outrages suffered by the Indians, a section that was later removed by the Senate.<sup>20</sup> While it is valuable to have a reprint of Kappler's rare collection of treaties, one must actually go to the *Statutes at Large*, the *Journal of the Executive Proceedings of the Senate*, the *Congressional Globe*, the *Reports of the Secretary of the Interior*, the *Congressional Record*, the *Annual Reports of the Commissioner of Indian Affairs*, and other government documents to ferret out the basic history of treaty negotiations.

The weakness of the Kappler volume might have been partially solved if the reprinting had included volume 1 of this two-volume work, which dealt with laws. Fortunately we need not concern ourselves with the defects of the Kappler volume when we now have a new four-volume compilation of government documents on native American history, Wilcomb E. Washburn's *American Indian and the United States: A Documentary History*. Washburn, probably our most versatile scholar of Indian-white relations, is well qualified to prepare a documentary history of the American Indian in our national history. His earlier one-volume compilation of sources, *The Indian and the White Man*, is among the most valuable single volumes on Indians and whites, covering the period from Columbus to modern times. Even more significant is the fact that in writing his recent book, *Red Man's Land, White Man's Law*,<sup>21</sup> Washburn familiarized himself with the corpus of legal and governmental documents he selected for his four-volume *Documentary History*.

Though Washburn's foreword to his four volumes is brief, it is instructive because he makes the point that Indian people possess a special legal status arising from race. In addition, he has chosen particular documents to illustrate the evolution of the special relationship between Indian people and the United States. His first volume begins in the year 1826 with reports of Indian commissioners. Volume 2 contains more reports of Indian commissioners from 1902 to 1963 and House debates on Indian affairs, 1830-66; volume 3 is composed of Senate debates on Indian relations, 1866-1907;<sup>22</sup> and volume 4, somewhat comparable to the Kappler volume, begins with the Fort Stanwix Treaty of 1784 and ends with a treaty of 1868 with the Navajos. Then follows a series of legal decisions spanning the years 1823-1973 and an index.

We must turn to the index to exploit this valuable collection of government documents. Here is a key to identifying interrelationships between

<sup>20</sup> "Colonel Henry Chivington and Colorado Territorial Politics," a paper being revised for publication by Sharon A. Boswell, a graduate student in history at the University of California, Santa Barbara.

<sup>21</sup> Washburn, *The Indian and the White Man* (New York, 1964), and Washburn, *Red Man's Land, White Man's Law* (New York, 1971).

<sup>22</sup> Washburn's editorial commentary on Indian treaties begins on page 2261 in volume 3 with the Delaware Treaty of 1778, which suggested the possibility of an Indian state and representation in Congress.

treaties, debates, and legal decisions. Utilizing the index is all the more important because Washburn has wisely rejected the saturation style of editing pioneered by Julian P. Boyd.<sup>23</sup> Instead, he has given us learned but brief introductions to larger sections as well as prefatory statements on individual documents. By following Washburn's introductions and the comprehensive index, the investigator can move efficiently to study such topics as treaties, the liquor trade, Alaskan native people, boundary questions, individual Indian tribes, the military, the Indian Claims Commission, individuals such as John Marshall, Henry Chivington, and Red Cloud, or such general themes as Indian removal.

Valuable as the index is as a research tool, there are few entries that will readily attract the scholar of interdisciplinary background. These volumes almost cry out for the compiler to steer us on to such topics as acculturation, assimilation, deculturation, environmental destruction, ecology, discrimination, factionalism, family, women, enslavement, conservation, genocide, racism, collectivism, population, ethnocentricity, ethnicity, stereotypes, violence, wildlife, and morality. Nor does the editor rely on such terms in his various introductory commentaries, although the editorial statements have commendable features, largely pertaining to the Indian and the law.

Indian people today know better than anyone else how important the law has been in their history. It has been twisted to deprive them of their religion—though not successfully—their water rights, their civil rights, their freedom of movement, their hunting and fishing rights, and finally their homeland. These documents accurately record the story of how native Americans were legally dispossessed. The tragedy, to be sure, is that next to outright warfare, the most successful technique for killing off Indians was to take away their land base. Land for Indian people, as well as most other tribal peoples, is a spiritual ingredient of their culture; it determines much of their social relationships; and they depend upon it for their livelihood. If there is a test of how well individual tribes survived, it is how well they held on to their land and withstood the alien white invasion.<sup>24</sup>

To see the underlying white attitudes behind this dispossession one need only wade through the documents in Washburn's *Documentary History*. Representative is Commissioner J. D. C. Atkin's report of 1886, declaring that "these Indians have no right to obstruct civilization and commerce and set up an exclusive claim to self-government."<sup>25</sup> As one would expect, Atkin was one of many hostile bureaucrats who took a hard line on reducing the Indian tribal land base in the name of civilization's westward march. Other documents in Washburn's edition consistently bear out the concept of

<sup>23</sup> For a discussion of the limitations of the saturation editorial method, see Brooke Hindle's review of volumes 16 and 17 of the *Papers of Benjamin Franklin* in *Journal of American History*, 60 (1974): 1071-73.

<sup>24</sup> Jacobs, *Dispossessing the American Indian*, 126-50. Douglas L. Oliver makes the same point in his discussion of Pacific island peoples in *The Pacific Islands* (rev. ed.; Garden City, 1961), xxii-xxiii, 157-73.

<sup>25</sup> Quoted in Washburn, *American Indian and the United States*, 1: 375.





*Fig. 2.* Chief Joseph (Hin-mah-too-yah-lat-kekht) of the Nez Perce about 1900. By Edward S. Curtis. Courtesy Huntington Library and Art Gallery.



progress so clearly described by Zolla in *The Writer and the Shaman*. More often than not, the documentary evidence shows whites were motivated by ideas of white racial superiority or by downright greed for Indian land and mineral rights rather than by philanthropy, as suggested by Sheehan. Though philanthropy was there, it was often consciously or unconsciously employed as an instrument for coercion, deculturation, and dispossession.

Collectively the new volumes on Indian-white relations throw a fresh beam of light on general American history, but we must be reminded that the lens needs continued adjustment to find the clearest image. Much of the new native American history comes to us from a shadowy level of historical perspective, from the bottom up. From such a vantage point, with the aid of cultural studies, traditional, elitist Anglo-Saxon themes are focused in sharp silhouette, often as romantic, racist images of the American past.

More than ever we realize that in addition to illuminating the shortcomings of traditional United States history, native American history can also throw light on admirable aspects of Indian society. We cannot help but respect the heroic stature of such native American leaders as Pontiac of the Ottawa, Tecumseh of the Shawnee, or Chief Joseph of the Nez Perce. Nor can we overlook the very permanence of Indian people. One obtains a feeling of permanence, a greater appreciation of the Indian's long past—and the implications for his long future—not only from reading about Indians but also from visiting, for example, the ancient Pueblos of the Southwest. This is a good feeling in a world of uncertainties. We know that the native American's history, as well as the history of this nation, is unfinished.<sup>26</sup>

<sup>26</sup> For an eloquent native American statement on the past and future, see "Visions of Spirit" and "Peace and Survival" by the Chumash Indian medicine man, Semu Huaute or Grandfather, and his followers, in *Redwind*, 1974, no. 1, pp. 2, 4-9.

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## Reviews of Books

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### GENERAL

FRANCIS KLINGENDER. *Animals in Art and Thought: To the End of the Middle Ages*. Edited by EVELYN ANTAL and JOHN HARTMAN. Cambridge, Mass.: M.I.T. Press. 1971. Pp. xxviii, 580. \$25.00.

In his investigation of the animal as subject in works of art, Klingender, a distinguished cultural historian, concentrated on the overt representation of animals as creatures in and of nature, on their diverse symbolic functions, and their participation as actors in fantastic or allegoric roles created by authors and artists. His premature death limited this still vast, thoroughly documented and illustrated book, carefully edited from the nearly finished portions of the text, originally conceived as a survey of animals in art from prehistory to the nineteenth century.

He tried to encompass the full range of man's experience of animals from hostile beast to domesticated servant, from real being to imaginary creature, presenting that experience as a metaphorical expression of man's reaction/abreaction to/from the natural world. Although Klingender did not take advantage of Leroi Gourhan's work on Paleolithic cave art, he effectively presented both the creation of the fantastic animal imagery of the ancient Near East with its protoheraldic patterns and the scientific naturalism of the classical world; the latter also developed a rich translational imagery of animals, charged with symbolic and didactic functions. The zoomorphic symbols of the barbarian Animal Style were seen as a counterweight to the absorption of classical animal imagery in the Christian art of the early Middle Ages with their emphasis on the Creation and the Flood. He then demonstrated the diametric conversion of this attitude in the apocalyptic tradition of the tenth and the eleventh centuries, opposed in its turn by the

humanist revival of classical themes and modes in Byzantine and Italian art from the eleventh to the thirteenth centuries.

The most important section of this fine book is its deep analysis of the transformation of animal imagery (fully represented in manuscript illuminations and in architectural sculpture) from those animals that described the perils of the soul in Romanesque art and literature to the naturalized creatures that responded to the developing scientific attitude of the Gothic age. This was the heyday of the beast fables, when the distorted authorities were challenged by the Aristotelian revival, soon capped by the animal studies of Albertus Magnus and Roger Bacon. Klingender beautifully characterized this change in his evaluation of Frederick II's great book on falconry, which contained sixty-three chapters on the natural history of birds as studied in their natural habitat by the emperor himself. This movement toward scientific naturalism was profoundly affected by St. Francis's positive and loving experience of the natural world, revealed by his *Sermon to the Birds* and his invention of the Christmas Presepio, where animals were again animals, friends of man, and like him sweet creatures of God.

Klingender's own *liber animalium* is a multifaceted mirror of human nature, of the exterior and interior landscapes populated by animals, as seen and depicted by generations of artists, and no less significantly by the author himself. It is both a definitive work and a delight.

RICHARD BRILLIANT  
Columbia University

CYRUS H. GORDON. *Riddles in History*. New York: Crown Publishers. 1974. Pp. 188. \$7.95.

Cyrus H. Gordon, who identified the earliest Minoan writing as Phoenician, is an outstanding specialist in early Semitic and Scandinavian

languages. Alf Mongé is a Norwegian cryptanalyst, to whom Gordon has dedicated his book. The pair has authenticated four texts generally called forgeries because the languages used did not conform to standard texts, because some words were misspelled, or both. Gordon shows that some of the texts belonged to previously undocumented historical periods, and Mongé shows that the spelling "errors" were deliberate keys to coded messages. At least the name of the scribe or runemaster, and the date, were usually concealed. The use of cryptograms (riddles) goes back to ca. 700 B.C., was lost during the Middle Ages, and rediscovered in 1969, before and after the dates of the alleged forgeries.

Of the four documents, the first is the Paraiba Stone found in Brazil in 1872, which bore a Phoenician inscription. A Mr. de Costa sent a copy of the text to Ladislau Netto, director of the National Museum at Rio de Janeiro, without locating the stone. The inscription states that Hiram III of Sidon (553–533 B.C.) sent a fleet of ten ships around Africa. One became separated and landed on the Brazilian coast. The text is a cypher, probably carved by a Jew who worshipped Yahweh secretly and Baal openly.

The Yale Library's Vinland Map, the second document, is supposed to have been drafted about 1400 in or near Basel. It was published in 1965. Although supposedly pre-Columbian, it clearly shows Greenland and northeastern North America, including Vinland. The Latin legend relates how Bishop Henricus (Eiríkr Gnúpsson) was sent to Vinland by Pope Paschal II (1099–1118) to study and report on that colony. Whether Yale has the original or a later copy is unimportant, because the Latin text is a cypher, which could not have been known to a forger until 1969, five years after the map's publication. Its code word is Lysander, a Spartan general recalled from a distant mission by two heralds carrying staffs on each of which was wound half of a message. Put together they ordered Lysander home.

The Kensington Stele is the third document. In 1898 Olof Ohman, an almost illiterate Swedish farmer, was clearing his land in southern Minnesota when he found a large runestone clutched among the roots of a sixty-nine-year-old aspen, making the undisturbed stone antedate the arrival of white settlers. Its date is Sunday, April 24, 1362. The text, which contains a cypher, tells how eight Goths and twenty-two Norsemen had sailed from Vinland, presumably via Hudson Bay, the Nelson River, and

the Red River, to a landing one day's journey from the stele site. Returning from fishing, they had found ten men bloody and dead.

The fourth document is the Spirit Pond Runestones. On May 27, 1971, Walter J. Elliott, Jr., a carpenter born in Bath, Maine, found three runestones in tidal water at Spirit Pond, Phippsburg, Maine. After many attempts to interest experts, Elliott finally got one stone to Alf Mongé and the other two to Cyrus Gordon. All three were cyphers, giving a date of October 6, 1123, the name Henricus, and that of Spirit Pond, HOOP, mentioned in the saga of Thorfinn Karlsefni. SP-1 also contains a still recognizable map of the region, including HOOP. These stones validate the Vinland Map and the Karlsefni saga.

Columbus arrived with firearms and the Spaniards founded viable colonies. The Phoenicians and Norsemen came without firearms and in small numbers, easy prey to the Indians. Yet Cyrus Gordon has proved their presence in the New World, whatever the cost in disbelief and ridicule such as others have suffered before him.

CARLETON S. COON  
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BURLEIGH TAYLOR WILKINS. *Hegel's Philosophy of History*. Ithaca: Cornell University Press. 1974. Pp. 196. \$7.50.

This book was written by a philosopher primarily for philosophers, although historians can learn much from it, not only about Hegel but also about some of the general theoretical problems involved in the interpretation and writing of history. Hegel was the greatest philosopher of history, Professor Wilkins says, but his philosophical problems "are not always or even usually the problems which contemporary English-speaking philosophers are primarily interested in." Wilkins attempts to build a bridge between the two; first, by making clear the main lines of Hegel's philosophy of history, then by examining some of the critical problems it raises in the light of contemporary philosophy, especially the views on Hegel advanced by Walsh, Findlay, Marcuse, and Popper, and Hempel's covering law model of explanation. This ambitious and original attempt in itself makes his book worth reading.

The unifying theme is how Hegel answers his own question: "What is the ultimate purpose of the world?" Chapter 1 is a full and lucid discussion of Hegel's division of historical writing into three varieties—original, reflective, and philosophical; of what each encom-

passes, how each relates to the other two, what each contributes to historical understanding, and why Hegel regarded the latter, speculative though it is, as superior to the other two. The second and third chapters deal specifically with Hegel's concept and use of mechanism and teleology, contingency and necessity, and his ideas about historical explanation. Although Wilkins's treatment of these thorny issues is too technical and involved to go into in a short review, he attempts throughout—on the whole, successfully, I think—to show the common ground between Hegel, despite his complexities, ambiguities, and indifference to the scientific verifiability of his position, and contemporary philosophers of history.

As for Hegel's well-known belief in the inferiority of natural science explanations of historical phenomena to his own, Wilkins ascribes it not to Hegel's ignorance of or contempt for science (a misconception), but to the primacy of freedom in his philosophy—to the fact that freedom was nothing less than Hegel's final answer to the question of the ultimate purpose of the world. "Spirit as self-determining is in need of no explanation beyond its freedom, which is its ultimate end." It is comforting to read that this position is still philosophically defensible and useful to the study of history, whether from a Hegelian or any other standpoint.

ROBERT ANCHOR

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QUINCY WRIGHT *et al.* *The International Law of Civil War*. Edited by RICHARD A. FALK. Published under the auspices of the American Society of International Law. Baltimore: Johns Hopkins Press, 1971. Pp. xix, 452. \$15.00.

The occurrence since 1945 of several far-reaching internal or "civil" wars invites scholarly attention and concern. In *The International Law of Civil War* a panel of scholars organized under the auspices of the American Society of International Law considers the impact of international legal rules on civil war. This is an important but difficult area of study; rules of international law governing the use of force are not easily stated with precision or agreement.

In this study the authors consider the relevance of rules of international law to the civil wars in the United States, Spain, Algeria, Yemen, Congo, and Vietnam. Some of these conflicts were wars of independence or self-determination; others were wars between politi-

cal elites to obtain or maintain control of the governmental processes.

Each author considers the relevance of rules of international law to the conduct of the armed conflict and to the participation of "outside" parties in the conflict. It is clear in some instances that the existing rules of international law were effective in setting the guidelines for participant behavior. Unfortunately the authors cite numerous instances in which the conflicting parties ignored appropriate legal rules. In other instances, rules of international law were employed by policy makers to accomplish prescribed political goals.

The authors indicate in a variety of ways the inadequacy and ineffectiveness of the traditional rules of international law regarding "outside" participation in civil strife in the contemporary period when there appears to be an interest among the participants to obtain "outside" support and assistance. Unfortunately there is no agreement among the authors regarding the content of new and more effective rules. Donald W. McNemar suggests that the postindependence war in the Congo established the precedent that unilateral military aid by an outside state to either participant in a civil strife is prohibited so long as the United Nations is deploying an international military force to control the conflict. Kathryn Boals offers as a new rule the "principle of modernizing intervention" that would support outside intervention in the conflict if there is a reasonable expectation that the intervention would contribute to the modernization of the local state. Although Boals's suggestion is rather startling on first reading, it deserves serious consideration.

The editor of the volume, Richard Falk, provides an excellent introduction in which he outlines the major problems in constructing legal rules to resolve the question of revolutionary violence and the construction of a stable world order.

Scholars interested in diplomatic history and the history of war will find this volume insightful and instructive. Most of the authors adopt a legal perspective as the dominant focus; Boals employs more of a social science approach as her dominant focus. None of the studies requires an international law background as a prerequisite for understanding.

DON C. PIPER

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College Park*

NICHOLAS PASTORE. *Selective History of Theories*

of *Visual Perception, 1650-1950*. New York: Oxford University Press. 1971. Pp. vii, 454. \$10.00.

Professor Pastore, who teaches psychology at the City University of New York, has produced a book that he hopes will be helpful to psychologists, and in this he is probably successful. His aim is to give an idea of what past and present theories of perception are about. In this field Pastore says that there are currently a multiplicity of theories and that their life expectancy is some ten years. By a careful examination of the theories of vision propounded by a selection of persons from Descartes on, Pastore hopes to illuminate the pitfalls in the field, a particularly difficult one that combines physics, physiology, and psychology, and where controlled experiment with humans is rarely possible. There are many examples of hypotheses based on experiments that are incomplete through lack of suitable subjects and undeveloped methodology.

The thoughts of some two-dozen distinguished philosophers and scientists are examined, from Descartes through Locke, Berkeley, Mill, and Helmholtz, to James, Köhler of the Gestalt school, Hebb, and Ames. It is claimed that the book adopts a case-study approach, but it would be clearer to say that the various theories are presented in digest form. It needs a modern scientist, an expert in the field, to interpret the ancient descriptions and theories, and this Pastore can do, but it is tough going for a student who is not himself fully conversant with the present state of the art, and here the word "history" in the title may initially lead some to expect a rather different treatment in the book. It is not a history, and those who have more recently published on the history of vision, for example, Alistair Crombie, Ernst Gombrich, Maurice Pirenne, are nowhere mentioned. As the book is completely internalist in organization, it would have been helpful to the non-specialist to have given an explanation of the key concepts involved in this branch of science. But that would be asking for a book aimed at a different audience.

G. L'E. TURNER  
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K. T. KHEBNIKOV. *Baranov: Chief Manager of the Russian Colonies in America*. Translated by COLIN BEARNE. Edited by RICHARD A. PIERCE. (Materials for the Study of Alaska History, number 3.) Kingston, Ont.: Limestone Press. 1973. Pp. xvi, 140.

S. G. FEDOROVA. *Russkoe naselenie Aliaski i Kalifornii, konets XVIII veka-1867 g.* [The Russian Population in Alaska and California, Late 18th Century-1867]. (Akademiia Nauk SSSR, Institut Etnografii im. N. N. Miklukho-Maklaia.) Moscow: Izdatel'stvo "Nauka." 1971. Pp. 269.

SVETLANA G. FEDOROVA. *The Russian Population in Alaska and California, Late 18th Century-1867*. Translated and edited by RICHARD A. PIERCE and ALTON S. DONNELLY. (Materials for the Study of Alaska History, number 4.) Kingston, Ont.: Limestone Press. 1973. Pp. x, 376.

Kiril T. Khebnikov's biography of Alexander Baranov, originally published at St. Petersburg in 1835, is printed in English translation for the first time. The author was employed by the Russian-American Company for over thirty years. After completing service in the company's headquarters in Irkutsk and as company agent at Kamchatka, he arrived at Sitka in 1817 where he assisted Baranov in terminating his administration as the company's first chief manager of the Alaska colonies. Khebnikov then served as company manager at Sitka under Baranov's four successors. He compiled the Baranov biography and other important colonial studies. Although the biography related nothing about Baranov's family, origins, youth, and early career and was uncritical and omitted data unfavorable to Baranov and the company, it described his administration and preserved many details that might otherwise have been lost.

The work was used by Ivan Petroff, who aided Hubert H. Bancroft in writing his *History of Alaska* (1885). It was a particularly valuable source of Hector Cheigny's popular biography, *Lord of Alaska: Baranov and the Russian Venture* (1942). Among other recognized Alaskan historians consulting Khebnikov's Baranov biography are Clarence L. Andrews, Clarence C. Hulley, Richard A. Pierce, and Stuart R. Tompkins. In writing about the history of sea otter hunting along the Pacific Coast, Adele Ogden and Harold McCracken have also used this first Baranov biography.

The translation of S. G. Fedorova's *Russkoe naselenie Aliaski i Kalifornii* (1971) by Professors Pierce and Donnelly is an important contribution to pre-1867 knowledge of Alaska history. The book is largely a historical and an ethnographic study of the Russians who settled on the Aleutians, in Alaska, and in northern California from the period of discovery until the purchase. The author discusses three stages of Russian penetration into northwestern



America. In the first stage, from 1743 until the chartering of the Russian-American Company in 1799, various merchant companies established settlements on the islands near the North American coast and in the Gulf of Alaska. In the second stage, from 1799 to 1819, the Russian-American Company colonized southward from the Gulf of Alaska, and in the third stage the company shifted its fur-hunting operations northward into the Alaska heartland.

The book's greatest value is its analysis of the geographical and social origins of the Russians who came to Alaska. Initially settlers were mainly state peasants from northern European Russian *guberniyas* and Siberia, but from 1799 to 1829 a majority were petty bourgeoisie from such Siberian towns as Tomsk, Tobol'sk, and Irkutsk. Actually, from 1799 to 1867 there was only a small influx of Russian colonists, and population growth resulted primarily from a natural growth of the Creole group. Other worthy features of this study are its treatment of Russian cultural influence upon the aboriginal population and of Russian educational, religious, scientific, and cartographic activities.

The study is based upon numerous archival sources, mostly unpublished, and upon many printed works of American, Canadian, and Russian scholars. The author consulted K. T. Khlebnikov's writings on the ethnography of the Russian population of northwestern America. Her research on Alaska was thorough, but it could have been deeper concerning California. She states that Fort Ross and the Farallon Islands were the most southerly Russian settlements, but she overlooked Russian penetration into the Santa Barbara Channel Islands. No mention is made of the fact that in 1815 Boris Tarasov and a party of Aleuts from the brig *Illmen* were stationed on San Nicolas Island for seven months.

The Russian edition of Fedorova's book contains fourteen plates with appropriate explanations. These depict Alaskan scenes from 1779 to 1837, except for figure 13, which is an 1817 map of Fort Ross. The Canadian edition has two additional plates—one of Sitka in 1860 and one of the coal mining settlement at English Bay, ca. 1858. The dust jacket of the Russian edition in paperback has summaries of the book's contents in both English and Russian; unfortunately, the English translation of the summary is inaccurate.

The simultaneous publication in English of the older Baranov biography by Khlebnikov and

of Fedorova's recent study as numbers 3 and 4 in the Materials for the Study of Alaska History series of the Limestone Press is a fortuitous contribution as the books complement and supplement one another. Whereas one remains an essential source for understanding pre-1867 Alaska history, the other becomes so. In both these books the editors have prepared glossaries explaining Russian names and terms.

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CHRISTOPH WEBER. *Quellen und Studien zur Kurie und zur vatikanischen Politik unter Leo XIII.: Mit Berücksichtigung des Hl. Stuhles zu den Dreibundmächten.* (Bibliothek des Deutschen Historischen Instituts in Rom, number 45.) Tübingen: Max Niemeyer Verlag. 1973. Pp. xix, 594. DM 134.

The author of this book has previously written a valuable account of the Kulturkampf in Germany. He has extended his work here to the field of papal history. The studies and sources indicated in the title consist of five large chapters, almost independent essays, organized more or less around three themes: the development of papal-curial government and policies under Pope Leo XIII in the 1880s and early 1890s, the relationship between the Vatican and the Italian government, and, most particularly, the relationship between the Vatican, the German government, and German Catholicism. Since the Vatican did not and could not make a sharp distinction between internal and foreign affairs, these themes constantly cross each other. Given the unavailability of the Vatican Archives for the pontificate of Leo XIII, and the fact that available sources do not permit us to draw a complete picture, difficulties Weber is fully aware of, the study is necessarily and inevitably somewhat fragmented, particularly since the book tends to concentrate on key personalities.

The author has done a prodigious amount of research in heretofore little exploited sources, and the result is a massive documentation in French, German, and Italian. This is both the work's major strength as well as its weakness. The source material is barely kept together by the text, and not all of it would be of equal interest to scholars in different but related fields. Some of the material is narrow, highly specialized, and of value only to those working in the minutiae of curial history. This does not make it an easy book to work through; the organization is cumbersome and vexing in



places, and the reader should be prepared for footnotes often running an entire page in length. But for those with stamina this is at the same time a very rewarding book that sheds light on such diverse topics as the Roman question, Catholic scholarship in the nineteenth century, and the papacy's tangled relations with German Catholics and the German government under William II.

The first chapter is a previously unpublished biography of Johannes de Montel, a member of the papal Rota and a key figure in settling the Kulturkampf. Successive chapters deal with the structure of papal government under Leo XIII, the Italian policies of the pope in 1886-87, and German-Vatican relations in 1891-93. One fact emerges quite clearly from Weber's presentation: the determination of Leo XIII to restore papal temporal sovereignty. This comes as no great surprise, but the degree to which the determination overshadowed all other issues is very interesting indeed. The Neo-Scholastic revival, for example, was guided less by considerations of philosophy or canon law than by the specific (Italian) political problem of justifying papal temporal sovereignty against the prevailing liberal ideology. Weber shows to what extent the pope's *Ralliement* policy in France, the slow but growing lack of interest of the Vatican in close ties to the German government in the early 1890s, and the interest of Leo in an understanding with the Italian government all were dictated by a desire to restore the temporal power. Weber then shows that the *Ralliement* was a failure, that the understanding with the Italian government never materialized (prevented by intransigents in the curia, whom the pope could not control, as well as by intransigents among Italian liberals), and that Leo's French policy contributed in a very real way to the "nationalizing" of German Catholics.

Despite the somewhat episodic and provisional nature of the work and the reservations noted above, this is an important study. It will have to be consulted in the future by anyone concerned with the history of the Vatican and the development of German Catholicism.

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S. L. VAN DER WAL, editor. *Officiële bescheiden betreffende de Nederlands-Indonesische betrekkingen, 1945-1950* [Official Documents Concerning Dutch-Indonesian Relations, 1945-1950]. Volume 3, 1 jan.-30 maart 1946. (Rijks Ge-

schiedkundige Publicatiën, Minor Series, number 38.) The Hague: Martinus Nijhoff. 1973. Pp. xxiv, 739.

IDE ANAK AGUNG GDE AGUNG. *Twenty Years Indonesian Foreign Policy, 1945-1965*. The Hague: Mouton. 1973. Pp. 640. 69 gls.

These two books, similar in their focus on Dutch-Indonesian relations, differ, however, in scope and in kind. Van der Wal's third volume of *Official Documents Concerning Dutch-Indonesian Relations, 1945-1950* is a further record of the tragedy of the Dutch effort to recover their once precious empire in the East. The 370 documents and about an equal amount of detailed notes and references that fill 694 pages in small type—along with four appendixes, two indexes, and one military map of Java and Madura—cover only the first three months of 1946.

The largest number of documents are, again, communications between Lieutenant Governor-General van Mook and Colonial Minister Logemann, despite the fact that van Mook was in Holland from December 15, 1945, through mid-February 1946 for consultations. But much space is also devoted to cabinet meetings in The Hague; to a great number of Indies officials' reports—and this time predominantly—on the Outer Islands; to official and unofficial meetings between van Mook and staff and Sutan Sjahrir, Indonesia's first prime minister, and members of his cabinet; and to communications between the Dutch foreign minister and Dutch ambassadors in London, Washington, and Melbourne, and between the British foreign minister and Sir Clark Kerr, special ambassador to Jakarta, as well as among the various Allied commanders in the area, particularly also on the withdrawal of the British troops and their replacement by Dutch forces.

The proposals of the Dutch government of February 10, 1946 (appendix 2) on the new structure of the Kingdom of the Netherlands, which suggested the formation of a federal Indonesia as an integral part of that kingdom together with the Netherlands, Surinam, and Curaçao, and the endeavors of van Mook to dispose gradually but determinedly of the Republic of Indonesia (p. 577), particularly the word "Republic" (p. 596), strikes flashingly with Sutan Sjahrir's demand for recognition of the authority of the Republic of Indonesia over the whole Indies. Sjahrir candidly admitted, however, that Indonesia will need the Dutch foremost among foreigners in Indonesia's reconstruction. Although van Mook realizes that

Sjahrir was genuine in this belief in the Dutch and thus came under heavy pressure from "extremist" groups, no effort was made to try to meet his demands in some way. "Jogja . . . lives in a dream world which can only be brought back to reality by a severe shock," van Mook said (p. 541). The tragedy that developed is elaborated more pointedly in Anak Agung Gde Agung's *Twenty Years Indonesian Foreign Policy, 1945-1965*.

It is Anak Agung's firm and explicit belief that the uncompromising and unyielding determination of the Dutch to keep claiming sovereignty over Indonesia has disillusioned the moderates and strengthened the "radicals" on the left. He elaborates on this extensively in relation to the question of West Irian: Dutch determination to retain sovereignty over the island made Indonesia's dominant political current veer further to the left and become heavily communist oriented at the decision-making level. Sukarno's diplomacy of confrontation (the military confrontation with the Netherlands and later the "Crush Malaysia" policy), his doctrine of the contradiction between the "new emerging forces" and the "old established order," became chief instruments in the formation of Indonesia's foreign policy in the 1960s. Thus Indonesia departed radically from her independent and active foreign policy laid down by Dr. M. Hatta on September 2, 1948, and became involved in a policy complementary to Communist China's aim of terminating American and British presence and influence in Asia. This break and the enactment of Sukarno's foreign policy came into the open at the Belgrade Conference of the Non-Aligned Nations (September 1-6, 1961). Nonalignment from this time on became identical with anti-colonialism. This new foreign policy concept served Sukarno's vanity tremendously: as he became a confirmed believer in the new twofold division of the world with himself as the sacred spokesman of the New Emerging Forces, he tried to secure for himself and for Indonesia "more prestige and greater voice in the councils of the world" (p. 445).

Anak Agung Gde Agung is well equipped indeed to analyze Indonesia's foreign policy incisively from its inception to the convulsions of 1965. His first-hand account of the interaction between domestic political disputes and power struggles and foreign policy issues, which runs from the period of Indonesia's first cabinet through Sukarno's Guided Democracy, is based less on carefully selected documents than on his personal involvement and reminiscences and

his extensive political experience as well as his socialist outlook. However, his analysis on specific issues and in some respects also on broader issues seems to draw quite heavily on views of foreign Indonesia specialists as well, on McTurnan Kahin, Herbert Feith, Donald Hindley, Guy J. Pauker, and others. Even so, Anak Agung deserves the honor of being the first Indonesian to write a voluminous and, in particular, captivating essay on Indonesia's foreign policy not only in English but in Indonesian as well.

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#### ANCIENT

MARIJA GIMBUTAS. *The Gods and Goddesses of Old Europe, 7000 to 3500 BC: Myths, Legends and Cult Images*. Berkeley and Los Angeles: University of California Press. 1974. Pp. 303. \$20.00.

In this book Professor Gimbutas attempts to reconstruct the mythology and religious symbolism of the region prior to the arrival of the Indo-Europeans. Old Europe, roughly an area extending from south Italy to the coast of Asia Minor and from Crete north to the Dnieper River, is held to have been a cultural unity during the period in question, from 7000 to 3500 B.C. This unity was destroyed by waves of successive Indo-European migrations, beginning in the mid-fourth millennium B.C. The culture of Old Europe survived in Greece throughout most of the third millennium (the Early Bronze Age) and was still to be found in Minoan Crete during the first half of the second millennium. This explains the numerous parallels between Neolithic Eastern Europe and Minoan Crete.

This book is essentially a study of two different bodies of material: idols or cult images and painted pottery. Most readers will find the treatment of the idols the more convincing. Gimbutas has made an interesting attempt to reconstruct the religious symbolism of the painted pottery of Old Europe, especially that of the Late Cucuteni period, but the results are so subjective that they lack conviction. It is doubtful that any independent researcher would reach remotely similar conclusions and find the same "rain torrents," "sonatas of becoming," "does in the shape of moon crescents," and "snakes winding across double-eggs enveloped in flowing water" in these highly abstract designs.

The discussion of the idols, especially the human ones, presents a wide-ranging body of material, much of it unfamiliar to me as I am more at home in the Aegean and in Anatolia. Just the superb illustrations alone (over four hundred), especially of idols from the Vinča period, make this book indispensable to anyone interested in the Neolithic period. Everyone will find something new here, and Gimbutas's comments on each idol are interesting and informative (as, for example, her remarks on "steatopygia," pp. 106-07).

Chronology still remains the most controversial aspect of European prehistory. Gimbutas is an enthusiastic supporter of the recalibration of radiocarbon dates and the high chronology for the European Neolithic. The problem is a critical one, involving differences of opinion that range over thousands of years. The date of the idols from the Tisza settlement at Szegvar, in southeastern Hungary, provides a good example of the situation. For Gimbutas the Tisza idols must be dated about 5000 B.C. (pp. 83-84, 128-29). For Ruth Tringham (*Hunters, Fishers and Farmers of Eastern Europe, 6000-3000 B.C.* [1971], pp. 186-87), who also believes in radiocarbon dates, the same material is placed in the early fourth millennium B.C. In the catalog to a recent exhibition of this material in Vienna, (*Idole: Prähistorische Keramiken aus Ungarn* [1972]), J. Csalog, the excavator of the site of Szegvar, dates the idols from 2800 to 2500/2400 B.C. ("Thronendes Frauenidol von Szegvar, Tüzköves," pp. 20-23). In this same catalog the following Tiszapolgár Culture (Early Copper Age) is dated 2500/2400-2300/2200 B.C. (by J. Korek, *ibid.*, p. 5). According to Ida Bogнар-Kutrian, the author of the recent monograph devoted to this period, the radiocarbon evidence indicates that these dates must be raised to 3600/3500-3300/3200 B.C. (*The Early Copper Age Tiszapolgar Culture in the Carpathian Basin* [1972], pp. 210-11). But for Marija Gimbutas the Tiszapolgár Culture covers the period 4500-3500 B.C. (p. 15). This, alas, is all too characteristic of the situation facing students of European prehistory today.

*Gods and Goddesses of Old Europe* is an excellent example of the handsome books now being published by Thames and Hudson in London. It is superbly printed, beautifully illustrated, and also, very up to date. It is remarkable to find, in a book published in early 1974, illustrations of material excavated—by the author—in August 1973. According to Gimbutas the over two hundred idols from this excavation (at Achilleion, near Farsala in Thes-

saly) date to ca. 6000 B.C. A brief note on the site is also included as a supplement to a very useful site index at the end of the book.

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CHRISTOPHER and SONIA HAWKES, editors. *Archaeology into History*. Volume 1, *Greeks, Celts and Romans: Studies in Venture and Resistance*. Totowa, N.J.: Rowman and Littlefield, 1973. Pp. xiv, 162. \$10.00.

This volume, the first of a projected series under the editorship of Professor and Mrs. Hawkes, brings together four substantial articles, three dealing with the frontiers of the Greco-Roman world, and the fourth with the Iron Age Celtic world of Britain and the Continent. David Ridgway's contribution deals with the excavations on Ischia, now demonstrably the earliest Greek colonial settlement on the Tyrrhenian coast of Italy. Excavations have been carried on under the direction of Dr. Giorgio Buchner since the 1930s, but only brief reports and descriptions of the most sensational finds (such as the eighth-century cup inscribed with an elegiac distich) have been published. Ridgway places the Euboean colonizing ventures at Ischia and Cumae in the context of their other ventures on the Syrian coast (Al Mina, Tell el Sukas) and discusses the evidences of metalworking by the Greeks on the island, the date of the settlement (before 750 B.C.), and the question of earlier relations of the Greeks with the native Campanians. Dennis Harding's article disposes of a long-standing assumption that British houses of the Iron Age were built on a circular plan while the continental type was distinctly rectangular by showing that both types are found in Britain and on the Continent. A reconsideration of the old excavations at Manchester and Templeborough is the subject of Grace Simpson's article in which she manages to fill in gaps in the topography and chronology of the sites. Finally, Johanna Haberl and Professor Hawkes discuss the toponymy and topography of Norcium in the light of the life of St. Severin by Eugippius.

The general appeal of this volume is more to the informed English public with an interest in the classical world and the Roman frontiers than to the American reader even with a learned background. The level of scholarship, however, and the archeological interest of the topic are unquestionable, and the printing of the volume is most attractive. The specialist in the areas treated will certainly consult this

book. Whether, as Hawkes suggests, the presentation of these essays in this format, rather than in scholarly journals, will make them more accessible is less certain. But his intent of focusing interest on the common ground of history and archeology, an implicit rebuttal, one feels, to the current surfeit of self-proclaimed scientific archeology, has made a volume of real value, and one may look forward with interest to its successors.

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EDMUND F. BLOEDOW. *Alcibiades Reexamined*. (Historia: Einzelschriften 21.) Wiesbaden: Franz Steiner Verlag GmbH. 1973. Pp. 90. DM 24.

Thucydides and Plato portrayed Alcibiades as a powerful and charismatic personality, and their picture is quite consistent with Xenophon's less sharply drawn account (not to speak of Plutarch's derivative one). Modern scholars have therefore been fascinated by this individual whose role was central during the last and catastrophic stage of the Peloponnesian War, and it has been taken for granted that he was a brilliant if egocentric man who might have saved Athens, perhaps even have conquered Syracuse, had it not been for his own perversity and the unfortunate circumstances that may have arisen from it. It has never been doubted that he was a master diplomatist and skillful general.

Bloedow's study serves as a corrective to this hagiology. His book is not a biography of Alcibiades but a series of critical studies, intended for the specialist, covering the events from the battle of Mantinea to the disaster at Notium, each episode being treated briefly in chapter form. It is indeed high time for some deflation of Alcibiades' ballooning image. Many grandiose claims have oversolicitously been made on his behalf, and it is Bloedow's contribution to have pointed them out. The objection, simply, is that Bloedow goes much too far. For example, he rightly condemns the frequent ascription to Alcibiades of the sole responsibility for the key dispatch of Gylippus to Syracuse and the fortification of Decelea in Attica (pp. 18-20), but he belittles unduly the importance of Alcibiades' advocacy of these measures. All of Alcibiades' successes now are alleged to have been won jointly with others (for example, pp. 46-55) or were failures in the long run (p. 36), while his reverses were all his own, whatever the circumstances (for example, pp. 72-79). He must even bear the responsibility for his recall

from Sicily (despite Thucydides' disclaimer). Bloedow here and elsewhere seriously underestimates the strength of Alcibiades' opposition at Athens. A multitude of hypothetical arguments, furthermore, about what Alcibiades would have accomplished if he had been a great general or master politician seems capricious.

Bloedow's detailed demolition of the extravagant assertions of others is valuable, but it is too thoroughpaced and makes inexplicable not only Alcibiades' influence with friend and foe alike but the pre-eminent position accorded him by Thucydides and Xenophon. The net result is that Alcibiades remains as pivotal as ever, even when demythologized.

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ROBIN LANE FOX. *Alexander the Great*. [New York:] Dial Press. 1974. Pp. 568. \$15.00.

Most Alexander scholars would feel suspicious of a book that the anonymous reviewer for the *Times Literary Supplement* called "the most sheerly original contribution to the subject since 1833." It is therefore to the author's credit that much of my initial skepticism was swept away by Robin Lane Fox's massive account of Alexander. This is the most exciting, if strange, book on Alexander to appear in recent years.

Lane Fox, a fellow of Magdalen College, Oxford, displays a comprehension not only of traditional Alexander scholarship but also of Asian geography, ethnography, and culture, subjects that the classical scholar normally shuns. Five hundred pages of closely printed text and fifty more of notes give the appearance of a learned tome. But the reading of the book produces the effect of a historical novel (some will call it that in any case). In a field often marked by lifeless prose, Lane Fox is a master of characterization and mood. The book also exudes confident opinion about a variety of things; for example, "Except for a love of gardens [the author writes on gardening for the *Financial Times*], there is no finer test of a civilized man than his taste for paintings." Some of us oenophiles would object, claiming rather that God resides in a glass of fine claret, and that the only proper use for a garden is to nurture vines.

There are some interesting defiances of scholarly convention. In one place: "I do not like . . . refutations of other men's views." In another: "To list my reasons for rejecting so many opinions would have required a companion volume and would not have concerned



the search for Alexander, the true historian's only business." And so, barely mentioning the most influential work on Alexander of recent times, he goes directly back to ancient evidence and writes from scratch. The influence of modern scholarship, while clear, is indirect, serving mainly to give focus to major historical and historiographical problems. Thus one comes away sensing that this is a fresh start on Alexander rather than a patchwork of current opinion.

But there is danger here. The author has made a religious exercise of his independence, causing him deliberately to slight or reject some very prominent views without telling why. Moreover, he often betrays his explicit refusal to engage in academic nitpicking by writing snipes that are usually gratuitous and sometimes insulting. This unfortunate tone weakens his case. One wishes that the manuscript had ripened and been pruned a bit.

The Alexander who emerges from this is an energetic young man, the product of a society dominated by martial spirit, imbued with his Homeric antecedents, creating myths about himself, spreading Hellenic culture to inner Asia, becoming an Oriental king, and maintaining full possession of his faculties right up to the end. It is an invigorating portrait, characterized by attention to detail and fascinating digressions on a variety of topics (there is more than a touch of Strabo or Pliny here). The volume is weak on administrative history, naive about logistics and communications, sometimes confused on chronology, and often wrongheaded about the tangle of ancient historiography on Alexander.

I am informed that Robin Lane Fox was twenty-seven years old when this book appeared. He is a young man writing about a young man, challenging modern scholarship as Alexander had challenged the institutions of his day, "searching" (his word) for Alexander as Alexander had searched the world for himself. It goes too far to suggest that Lane Fox is Alexander; indeed it is not clear whether he even likes his hero. But he often identifies with him, youth with youth, energy with energy. What results is the most personal (and original) portrait of Alexander since W. W. Tarn's appeared a half century ago. Whether it is the real (that is, historical) Alexander will, despite Lane Fox's protests, be a subject for scholarly debate. This is a major account, deserving to be read and begging to be criticized.

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RAMSAY MACMULLEN. *Roman Social Relations, 50 B.C. to A.D. 284*. New Haven: Yale University Press. 1974. Pp. ix, 212. \$8.95.

Ramsay MacMullen's work is always provocative and illuminating. This book, the author's fourth, is no exception. The topic is divided into three chapters based upon the grounds of social relations: rural, rural-urban, and urban. There follow a fourth on class divisions and a last, brief summarizing chapter. Appended are subsidiary discussions of urban subdivisions, ancient terms of snobbery, and city financing. The last is quite sketchy and must be supplemented by R. Duncan-Jones's *The Economy of the Roman Empire*, which appeared simultaneously. Its density of detail serves to illustrate several aspects of MacMullen's topic.

The principle on which MacMullen constructs his essay lies in the relative uniformity and continuity of the human condition over the three centuries and more of Roman history set by the title and within the geographical limits of Mediterranean culture in Italy and eastward. MacMullen holds, no doubt correctly, that repeated ancient utterances of like drift, but of different times and places, bespeak a certain cultural homogeneity. If caution must be applied, it results from the otherwise normal reliance on papyrus documents from Egypt, a land much different from all other segments of the Roman Empire in its administration, class structure, economy, and its stage of civilization. That said, MacMullen's choice of ancient voices and sentiments usually represents a society similarly stratified in West and East. The essay's brief compass shows how very simple life was in certain respects.

Through good writing, clear presentation, and outstanding common-sense judgment, the author has given us chapters to be read with pleasure by a large audience, specialist or not, that will learn much about the character of ancient society. MacMullen evinces particularly keen perception in rendering sensible to us ancient notions of class and the known limits of the several classes. Thus he puts in doubt any recent thesis of an ancient "middle class" that might have drawn great, or even modest, wealth and respectability from commerce and manufacture.

This book exhibits a wide reading in the least-approached sources and a capacity to enucleate the best from the latest research. MacMullen's oblique remarks on the social views of ancient historians, especially the unlovable and vicious Tacitus, give joy, at least to me, even though we must ever bear in mind that

the historian in antiquity was educated to write for the thinnest of all the social strata, the senatorial aristocracy. So it was and ever will be, thought the historian, Rome without end.

This fine book represents for us what we may legitimately know of ancient society, and thus it has the salutary effect of countering works on like subjects that presume to indoctrinate us by asking why Romans, Greeks, Syrians, or Egyptians did not produce the likes of Karl Marx.

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MORTON SMITH. *The Secret Gospel: The Discovery and Interpretation of the Secret Gospel According to Mark*. New York: Harper and Row. 1973. Pp. ix, 148. \$5.95.

MORTON SMITH. *Clement of Alexandria and a Secret Gospel of Mark*. Cambridge, Mass.: Harvard University Press. 1973. Pp. x, 452. \$30.00.

The first of these two books is an appealing, even moving, but popularized report of the discovery of the letter of Clement of Alexandria, which contains quotations from a Secret Gospel of Mark. For serious discussion, however, one will have to turn to the second book, which contains the actual publication of this important new text (page references are to this latter volume). It gives a careful commentary on the whole text, including several lengthy notes, and, after detailed discussions of the Secret Gospel and its background and history of its text, a number of very valuable appendices. (The extensive presentation of the relevant source materials, especially about the sect of the Carpocratians, will be appreciated by any reader.)

The evidence presented for the authenticity of the letter seems to be conclusive. Its style and vocabulary are Clement's; as Clement, the letter reveals an ambiguous position toward Gnosis, values the secret tradition very highly, but is critical with respect to the abuse of such traditions by libertine Gnostics. Smith also gives impressive evidence for the typically Markan style and vocabulary of the fragments of the noncanonical "Secret" Gospel of Mark that are quoted in this letter. Only a few elements remain which could point to some Lucan influence (compare the phrase "for he was rich" from Luke 18:23, p. 114, and the Lucan traits discussed on p. 121: are these later glosses?). There can be little doubt, the Secret

Gospel of Mark, of which Clement is discussing several passages, was a Markan Gospel that contained pericopes not present in the canonical Mark. The comparison of the raising miracle (quoted by Clement from this longer text of Mark) with the story of the raising of Lazarus from John 11 demonstrates that the former represents a more primitive version of the same story (pp. 152-58). Thus, one must also agree with Smith's conclusion: this longer text of Mark and the Gospel of John used a common source, probably a written source from which all the miracle stories of both Gospels derive.

Smith himself is doubtful with respect to his own hypothesis that the material in the longer text of Mark is systematically arranged in such a way that the same liturgical pattern is repeated three times in Mark 8-9, 10, and 14-16 (pp. 165-67). But that the text of Mark 10 as it appears in the Secret Gospel is a coherent baptismal pericope is very suggestive (pp. 167-88). In this context, Smith's investigation of the term *mysterion* is significant (pp. 178-83). He brings impressive evidence from Christian, pagan, and Jewish sources to demonstrate that the term can also mean "secret rite" (not only "secret teaching"). But one wonders whether such meaning can really be assumed for the occurrence of the term in the genuine Pauline Epistles—Ephesians and Colossians are different. With respect to the meaning of *mysterion* in Mark, Smith's understanding of "secret rite" is acceptable only if one follows his emendation of the longer text to read, "he gave him the mystery of the kingdom" instead of "he taught him the mystery" (p. 183). Furthermore, if the editor who produced the canonical text of Mark omitted the pericope in Mark 10 because it referred to the "secret rite" of baptism (inappropriate for an "exoteric" book), why did he not also delete the account of the equally secret rite of the Lord's Supper in Mark 14, which contained the top-secret words of institution (pp. 187-88)?

This raises several far-reaching questions about the implications and conclusions drawn from this new Gospel text. To this the author devotes a long chapter of the book (pp. 195-278). The early date for the document is certainly correct. But a date somewhat before 125 A.D. is still about half a century later than Paul and belongs in the second to third generation of Christianity, even if this new document—and here I agree—reflects a text of Mark that is more primitive than the canonical text of this Gospel.



How early were such Christian rituals called mysteries? And how early and how widespread was the distinction between "esoteric" and "exoteric" writings or teachings? The answers must be specific. Paul's opponents may have transmitted sayings of Jesus as "mysteries," and they may have connected them with the rite of baptism. But we do not know whether or not they called this rite a "mystery." Paul dissociates himself and his proclamation from baptism (I Cor. 1:13-17) and emphasizes that "mystery" is eschatological knowledge that includes the confession of the crucified Jesus as eschatological Kyrios. Baptism is the entrance rite into the Christian community but not into the kingdom of God (against Smith, p. 203)—in Paul the two are not identical. If Paul's understanding of baptism derives from the most primitive period of the Church, there is no basis for the conclusion that Jesus has instituted a secret rite of baptism as admission to the kingdom (pp. 213-16). Whatever the historical Jesus thought about baptism—only fundamentalists would agree that Jesus claimed to be the Messiah (p. 235) and that therefore baptism would unite the believer with him, the Messiah—according to Paul, the members of the community participate in Jesus' death and resurrection through baptism. Thus, Christian baptism appears as a "post-Easter" rite; the receiving of the Spirit is related to a post-Easter event, not to the life and activity of Jesus. Only the Eucharistic meal was seen as instituted by Jesus himself; but in its essence it was an eschatological meal anticipating the Messianic banquet. This is still true for Paul, *pace* its magical connotations. That it became a magical meal in which blood and flesh of the Savior were distributed is obvious a generation after Paul, i.e., in the time to which Smith assigns the Secret Gospel of Mark.

Smith's discussion of magic and magical rites in that period of antiquity is fascinating (pp. 220-37). One should not doubt that his comparison of Christian sources with other ancient texts (also Palestinian Jewish materials) uncovers a great deal of relevant data even in canonical Christian writings. But, as Smith states (p. 235), "Magician( was a dirty word," and the ancient debate whether such phenomena should be called "magical" (a term used by opponents) or "evidence of divine presence" (thus the believers) must not be repeated today. Different tools of critical differentiation are needed. Otherwise all those events and experiences which transcend the realm of whatever any generation chooses to call "natural" or

"normal" would be easily and in wholesale fashion classified as "magical" or "superstitious"—a term which already the Romans used to characterize anything that did not conform to accepted practices of civil religion ("superstition" is Pliny's accusation, no word about magical rites, *pace* pp. 234, 254-56). Also modern "scientific" terms must be chosen carefully. Smith characterizes "speaking in tongues," "salvation by possession," and "inspiration" as symptoms of schizophrenia. But such phenomena belong to the psychological abnormalities known as hysteria. A history of religions approach must clarify, first, the widespread beliefs of the Hellenistic and Roman period in manifestations of divine and supernatural power; "magic" is only a polemical subcategory. Homer composed his epics by divine inspiration, not through magic. Second, the forms and genres of transmission and communication of any such events (e.g., miracle stories) must be investigated in view of their sociological function and tendencies of interpretation; Smith does this admirably well with respect to esoteric communities such as the Gnostic sect of the Carpocratians, but generalizations are of limited value. Third, the "objective" description of the phenomena themselves ("magical rites" or "spirit possession") remains preliminary; historical insights cannot be gained unless one considers the creative contribution of such occurrences to the life of the society in the context of its conflicts, anxieties, and expectations.

All these perspectives are conspicuously absent in Smith's treatment of the assumed intention of the secret baptismal rite: ascent to the heavens (pp. 237-48). Paul knows that such experiences are without any sociological implications (II Cor. 12:1-10; Col. 2:9-15 is not Pauline). Also the transfiguration story of Mark 9, however esoteric, does not illustrate the consequences of an initiation rite, and it defies any conclusions with respect to the historical Jesus (against pp. 243-44). The logical connection between "ascent to the heavens" and "freedom from the law" is not clarified (pp. 248-51). The discussion is dominated by the assumption that the only alternatives to legalism are sinfulness and immorality (pp. 258-62). There is no question, to be sure, that many people then and now would agree with such limited alternatives; historically, Christianity was confronted with many more choices, ranging from a limited and modified concept of legalism to the freedom of a "new morality." It would be necessary to specify such options and their social and political consequences in

the context of the crisis of morality in the Roman period. In fact, as Paul's correspondence with the Corinthians demonstrates, the question of the Law does not necessarily play a role in a debate about morality (against Smith's assumptions, p. 257).

In conclusion, I can only confess that this work belongs to the few monographs in this field that are truly substantial, exciting, and challenging. No student of the New Testament and of the history of early Christianity can afford to neglect the significant new text published here, nor should anyone try to avoid a discussion of the issues this book raises and debates on the basis of unparalleled knowledge of such a variety of relevant source materials.

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### MEDIEVAL

COLIN MORRIS. *The Discovery of the Individual, 1050-1200*. New York: Harper and Row. 1972. Pp. xviii, 188. Cloth \$5.00, paper \$3.45.

Professor Morris, drawing heavily on the methodology of Burckhardt, has in this book added a new dimension to our understanding of what is usually known as the twelfth-century renaissance. His net is cast wide to include, for example, the practice of confession, portraiture, atonement theology, chivalric literature, and hagiography. Concrete illustrations abound, and as a result the reader's interest never flags.

While doing scant justice to the rich variety of instances that supply the book's main interest, a brief summary may be useful to the potential reader. After stating "the question" in an opening chapter, wherein it is made clear that we are to have no Burckhardtian political framework, there is a chapter on origins in the late Carolingian and the Ottonian ages. Social and religious change are briefly discussed as a setting for "the discovery" in chapter 3; the question of classical origins as against new departures, recurring throughout, is adumbrated. Chapter 4 is meaty, dealing with the quest for self-knowledge in preaching, confession, psychology, autobiography, and portraiture. The theme is then developed in three chapters devoted to the self and other selves, a discussion of friendship and love, the individual and society, which deals with satire as the "path of rejection" and with the path of reconciliation as exemplified in the romances of Chrétien de Troyes, and the individual and his religion, which deals with personal religion

in matters of Christology, eschatology, and mysticism. In this chapter Morris's evident religious awareness brings a special richness to the discussion. A conclusion brings together the main themes and findings.

While falling short of Burckhardt in translucent clarity, Morris usually shows acute discernment and original insight in his observations. His happy use of concrete illustration is shown in the following example. Writing of the new personal element in the vision of Jerusalem, the closing lines of Hildebert's *Hymn to the Trinity* is quoted (in the original and in translation) with the following conclusion: "It is interesting, and also moving, to observe the same phenomenon here as in Walter of Chatillon. Both poets, although men of the new age, with a greater confidence in the regularity of the created order, have lost the sense of community and also the immediate sense of God's presence. Hildebert can salute the heavenly city only from far off, *de longinquo*. The same is true of Abelard. His tremendous sense of the power of the human intellect never effaced his final loneliness. In the most famous of all Jerusalem hymns, *O quanta qualia*, there is no trace of the old eschatology. Jerusalem no longer comes down from heaven, as in the Scriptures; the coming of the Son of Man to make all things new has no part in Abelard's devotion. Lonely, the exile longs for home:

'Now in the meanwhile, with hearts raised on high,  
We for that country must yearn and must sigh,  
Seeking Jerusalem, dear native land,  
Through our long exile on Babylon's strand.'

It is in this manner that the author builds up his argument.

This little book will elicit criticism both of the general theme and the particular interpretations. But its main value will be to stimulate emulation among its readers. The wise teacher will use it to encourage students to go to the originals (of which a selected bibliography of translations is included) to elaborate, confirm, refute, and to open up related paths of inquiry. No better tribute could be paid to this original and imaginative study than that it will stimulate students for a long time to come.

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ROBERT B. PATTERSON, editor. *Earldom of Gloucester Charters: The Charters and Scribes of the Earls and Countesses of Gloucester to A.D. 1217*.

New York: Oxford University Press. 1973. Pp. xxiv, 205, 32 plates. \$32.00.

The editor's first sentence provides a concise description of the contents as "the *acta* of the lords and ladies of an English secular feudal barony, the Earls and Countesses of Gloucester, from the early twelfth century until 1217." The principle of selection was to include "all known charters issued in the names of the earls and countesses," a principle that excludes any charters addressed to them. Because the future King John and Geoffrey de Mandeville also held other lands when they held the earldom, only their charters that deal with the earldom are included.

To a medieval historian in this country there is a strong predisposition to welcome the publication of two hundred charters from manuscripts scattered in various repositories in England, most of which have never been published in any form, provided only that the editing is carefully done. For this book the welcome can be unreserved. Professor Patterson wisely adopted the model developed in publishing royal charters of the Norman kings of England. He provides a brief technical description of each charter, a transcription, and brief annotations that mostly concern criteria for dating. Although the facsimiles that are printed are designed to illustrate the hands of various scribes, they are numerous enough to provide a fair idea of the original charters and a check on the accuracy of the transcripts. An appendix of references to other charters that no longer exist and an index of persons and places add significantly to the value of the edition. Although organizing the charters by the beneficiary is a practical necessity in a period when charters were undated and can be dated only within a broad range of years, the historian interested in which charters were issued within which chronological context will simply have to sort through the book on his own.

Of course, the most obvious historical value of the charters is the information they contain about the earldom. The editor is engaged in a historical study of the honour and earldom of Gloucester and offers this edition as preliminary to the larger task. The charters contain information on a great variety of other topics and individuals, however. Grants to monasteries are especially prominent. When a reviewer finds new information on the subject of his current research, he can only applaud the publication of these charters, but one must take potluck, and the major value of this edition for most historians may well be in the

introduction. Here the editor works out how the earls of Gloucester adopted an administrative organization similar to the royal administration, and his description of the proto-chancery of a feudal baron provides a unique case study.

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ANTHONY TUCK. *Richard II and the English Nobility*. New York: St. Martin's Press. 1974. Pp. viii, 255. \$16.95.

Anthony Tuck, who has already published several important articles on this period, now gives the results of his investigations into Richard II's relationships with the nobility. In fact, the book has a wider scope than its title indicates, and he makes substantial modifications to the views expressed in the standard works of T. F. Tout and A. B. Steel.

Although Edward III, by deliberately enlarging the numbers of the titled nobility by elevating to the peerage men of suitable social status, and refraining (to his own financial disadvantage) from interfering with their property dispositions, achieved that harmony between monarchy and aristocracy which was the most successful type of medieval government, his reign ended in anticlimax and dissension—so much so that the chronicler Walsingham welcomed Richard II's accession as the dawn of a new and brighter era.

Tuck rehabilitates the reputation of the continual councils of the minority and gives a more favorable account than most historians of John of Gaunt's activities during this period. Passing somewhat lightly over the Peasants' Revolt of 1381, he convincingly demonstrates that during the 1380s Richard alienated the established nobility by lavishly endowing new nobles of lowly origin, cutting off the older groups from both influence and royal patronage—the latter supremely important for them in the maintenance of their own affinities.

In the Merciless Parliament of 1388 the rebellious Appellants were able to rely on the support of the Commons who had their own bitter grievances against royal extravagance and heavy taxation. The Appellants, however, formed a shaky coalition. They failed to live up to the Commons' hopes. The Commons, therefore, turned back to the king, one of the main reasons for the switch being apparently the opposition of the aristocracy to any restraints upon retaining livery and maintenance.

According to Tuck, Richard all his life

feared for his position, and the aggressive actions of 1397 onward, which finally brought about his downfall, sprang from an intensification of these fears rather than from a desire for revenge against his old enemies of 1386 to 1388.

Tuck's analysis carries conviction, except perhaps at one point—the king's responsibility for the events of 1381 to 1386. Born in 1367 he was surely too young to have planned his own policy during these years, as the author consistently implies.

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H. B. TEUNIS. *Crisis: Studie over een structuur- en normverandering in het Frankrijk van ca. 1150–ca. 1250*. (Bijdragen van het Instituut voor Middeleeuwse Geschiedenis der Rijksuniversiteit te Utrecht, 35.) Groningen: H. D. Tjeenk Wilink. 1973. Pp. 204. 24 gls.

This interesting analysis of changes in the political structure of France, particularly in terms of royal power, in the reigns of Philip Augustus and Louis IX is in fact a thesis presented for the degree of Doctor of Letters at the University of Utrecht where, as elsewhere on the Continent, regulations require such theses to be printed and published. In this case one can only applaud a rule that has made available such a significant contribution to our knowledge of European history in a critical period. At the same time it is sad that so few English-speaking medievalists will be able to read Teunis's work: the French and English summaries are useful but too brief to do it full justice.

Some may find it annoying to have the notes to this punctiliously documented book at the end, but at least they are concise, clear references, neither interlarded with *op. cit.*'s and *ibid.*'s nor extended by gratuitous advice about further reading. The author's style is as charming as his approach is fresh and unstuffy, and he analyzes his main sources of information deftly and to good effect. They are Rigord and William of Nangis, monks of St. Denis, William of Brittany, Gilbert of Tournai, and other contemporary writers. Against the ideological background of these chroniclers Teunis describes the twelfth-century structure of shifting baronial coalitions and shows how it was replaced at the end of the century by a fundamental conflict, between Philip Augustus and his most powerful vassal Henry II, that split France in two. The resulting crisis of 1202–04

introduced an element of discontinuity into the development of royal power, indeed facilitated a veritable break-through, which does much to account for the prestigious and nearly unchallenged monarchy of Louis IX. In sum, this is a technically admirable and historically outstanding piece of work from a thriving Institute of Medieval History, which deserves to be much better known in the English-speaking world than it is.

RICHARD VAUGHAN

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WILLIAM MENDEL NEWMAN. *Les seigneurs de Nesle en Picardie (XII<sup>e</sup>–XIII<sup>e</sup> siècle): Leurs chartes et leur histoire. Étude sur la noblesse régionale ecclésiastique et laïque*. In two volumes. (Memoirs of the American Philosophical Society, volume 91, parts 1 and 2; Bibliothèque de la Société d'Histoire du Droit des Pays Flamands, Picards et Wallons, volume 27.) Philadelphia: American Philosophical Society. 1971. Pp. 358; 427. \$25.00 the set.

Although William M. Newman's contributions to medieval history are already great, *Les seigneurs de Nesle en Picardie* represents his true life work, a monumental proof of his vast erudition and steadfast commitment to rigid and unwavering canons of historical scholarship. Only he could possibly have seen through such a project to successful completion.

Few scholars have ever equaled Newman in his insistence that all history must be grounded in fact, indeed that history is fact and that any departure from the concretely ascertainable must be condemned as an adulteration of historical purity. As he puts the case himself after referring to an illustration drawn from another, though neighboring, region: "But it is preferable not to generalize and not to cite examples from a diocese we have not studied."

Hostility to generalization provides the key to understanding this book. Newman's goals are concrete: to come to grips with one noble family of regional importance during the twelfth and thirteenth centuries, and especially to elucidate that family's connections with surrounding bishoprics and cathedral chapters at a time when all relationships were being profoundly modified by the economic, religious, and political changes of the age. For it is his firm conviction that "the connecting thread of history, viewed generation by generation (and not falsified by a grand overview of the whole dictated by the idea of development), was the spirit of family, which overshadowed all else."

To achieve these goals, Newman first edited

the 235 acts or mentions of acts that comprise the bulk of volume 2. Themselves drawn from fifty-five separate repositories in three countries, they represent all that is known directly about the seigneurs de Nesle in the period 1115–1286 and, as such, they provide the evidentiary support for the data presented in volume 1. As was to be expected, the editorial scholarship is impeccable.

Nevertheless, volume 1 and the historical philosophy underlying it will undoubtedly give rise to intense debate. Far from being a history of the seigneurs de Nesle in any conventional sense, it consists largely of genealogical tables and chronological lists buttressed by exhaustive references to the documents of volume 2. No interpretive conclusions are drawn; instead, the facts are allowed to speak for themselves. And one can only marvel at the painstaking attention to detail that informs every page of these studies.

At the same time, however, one has one's doubts. Because Newman's goals are so well defined and so precisely limited, the whole enterprise takes on a strongly eighteenth-century antiquarian flavor. One has visions of the legendary Talmudic scholar crushed by the weight of his authorities, or of the historian become Cheshire cat as he disappears inexorably into the branchlike maze of his scholarly apparatus. Such are the risks that Newman has run, but he did it knowingly, unhesitatingly, and driven by a profound sense of conviction. I would hesitate to say that he did so unwisely.

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RICHARD VAUGHAN. *Charles the Bold: The Last Valois Duke of Burgundy*. New York: Barnes and Noble. 1974. Pp. xvi, 491. \$28.00.

With this volume Professor Vaughan completes his study of the Valois dukes of Burgundy. Of all the books in the series, this is the most peculiar. It is not surprising that the author gives little space to Burgundian institutions under Charles. Vaughan did not pay much attention to institutional history in the earlier volumes and could argue that Charles merely continued the system of government described in the book on Philip the Good. It is a little more surprising to be told that an examination of ducal accounts was "a task beyond the scope of this work" (p. 407). But the greatest surprise is to find Charles treated primarily as a prince of the Empire. Up to a point this is an excellent idea. Charles's politi-

cal ambitions and military activities were concentrated in the Empire. His greatest successes were at the expense of German princes, and his ultimate defeat was caused by a loose alliance of German and Swiss towns, German bishops, and the duke of Lorraine.

The only problem with this interpretation is that Louis XI scarcely appears in the narrative. Philippe de Comines is certainly unreliable, and earlier writers have undoubtedly exaggerated Louis's role in Charles's downfall. Yet Vaughan tells us that Charles feared and hated Louis. There must have been some reason for this hostility, but it is never explained. Louis remains a distant, brooding shadow.

The best parts of the book deal with diplomatic and military history. A number of interesting documents are translated, notably reports from ambassadors sent to Charles's court. The organization of the Burgundian army is clearly described. Vaughan has surpassed all his predecessors in his careful and accurate descriptions of the crucial battles at the end of the reign.

One also gains a new and more satisfying idea of Charles's character. He had real ability as a military organizer and considerable skill as a diplomat. He could make long-range plans and wait patiently for them to come to fruition. But he hated to change his plans or his basic political concepts. He could not abandon his hostility to urban communities, and in the end the towns destroyed him. He worked hard to ensure a steady flow of Italian mercenaries for his armies, and in his last battle the commander of these mercenaries betrayed him. Once he had started a campaign he could not stop, hence the useless siege of Neuss and the needless defeat at Nancy. Charles's nickname should not have been the Rash or the Bold. He was Charles the Stubborn.

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A. LYNN MARTIN. *Henry III and the Jesuit Politicians*. (Travaux d'Humanisme et Renaissance, 134.) Geneva: Librairie Droz. 1973. Pp. 263.

Although the subject of this book seems, at first glance, to be fairly narrow, it opens up many windows. The France of the religious wars assumes dimensions that are more understandable, as we follow Henry III from his militant anti-Huguenot role as crown prince to his efforts as king to maintain a balance and preserve the peace. We see some of the many pressures upon him. Not least interesting are



his religious devotions, apparently influenced by the Jesuit Edmond Auger, which seem out of character with the degenerate king often portrayed. Martin carefully avoids psychoanalyzing the dead, but he explores the possibilities behind this strange behavior and finds it, in part at least, quite rational within Henry's frame of reference, although it won him no friends even among devout Catholics.

Then there is the international scene. Much of the book rests on correspondence from France to Rome, but letters from parties in England, Scotland, the Netherlands, and Spain also provide information. It was a tangled maze of competing interests, as we have known. But understanding the complexity in general is no substitute for the nitty-gritty give and take on specific issues that Martin offers. Two of the most intriguing chapters deal with "L'Impresa," the plot for a military invasion of England in the interest of a Catholic restoration. Mary Queen of Scots, James VI, Mendoza, Henry of Guise, Philip II, and the pope, with Jesuits acting as couriers and *provocateurs*, ran into financial problems and the reluctance of Henry III to encourage an enterprise that inevitably would have increased the power of Guise and Philip. In general terms this story has been known, perhaps, but here we are enabled to follow much of it blow by blow through the correspondence.

Finally, the book offers an extremely valuable picture of the activities of the Society of Jesus. Without rejecting the myths out of hand, Martin qualifies each of them. While some of the Jesuits were involved in political intrigue (the evidence is here in abundance), most were not. While the order did exert a strong discipline in some respects, the general at Rome was often quite unable to have his orders carried out. Often he had to rely on conflicting reports or downright prevarication. Among the Jesuits of France, in particular, there were two distinct factions. The image of a machinelike efficiency in the Society thus gives way, at least in France, to the recognition of personality and nationality.

Martin has spent the necessary time in the European archives and has taken full advantage of the major collections of printed sources. This book should go far to further not only the author's reputation as a scholar but also, perhaps, the reputation of Americans engaged in French studies.

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ANTONIO LINAGE CONDE. *Los orígenes del monacato benedictino en la Península Ibérica*. Volume 1, *El monacato prebenedictino*; volume 2, *La difusión de la "Regula Benedicti" en la Península Ibérica*; volume 3, *"Monasticon Hispanum"*; *Apéndices*. (Colección Fuentes y estudios de historia leonesa, 9-11.) León: Centro de Estudios e Investigación "San Isidoro." 1973. Pp. xx, 474; 489-1100; 590. 3,000 ptas. the set.

Dr. Linage Conde's work marks a major step forward in the clarification of early Spanish monastic history, and—because of the central importance of monasticism in Spain—of Spanish history in general. It directly concerns one of Spain's perennial problems: to what extent the country is part of Europe. Few doctoral theses have made so solid a contribution. As a disciple of Professor Díaz y Díaz, Linage has been trained in codicology as well as diplomatics. His mastery of modern research as well as of the original sources is apparent not only in the forty-seven page bibliography but throughout the book.

Linage sets the stage by discussing the early diffusion of the Rule of St. Benedict of Nursia (d. ca. 550) and monasticism in Spain before the Islamic invasion of 711, when Spain was not exceptional in its failure to observe the Benedictine rule—the rule only attained exclusive authority for monks in any part of Western Europe in the eighth century. In Spain, as elsewhere, monks lived under a regime of *codices regularum*. These collections, comprising rules by different authors, often included Benedict's rule. Textual parallels indicate that his rule was known, probably in this way, to Spanish monastic legislators before 711.

The main part of Linage's book is concerned with the period from 711 to 1109. Carolingian Europe accepted the Benedictine rule as the one authority for monks. It was adopted in Catalonia in the early ninth century when that region was incorporated into Charlemagne's empire. The rest of Spain, in this, as in other ways, continued unchanged. In the Islamic south the rule was known to at least one Mozarabic writer. It was adopted in 905 by the Leonese monastery of Abellar, probably founded by Mozarabic immigrants from al-Andalus. In the northwest, in general, from Galicia to Castile, the rule had to contend not so much with other monastic codes as with a constantly changing frontier society, antagonistic, by its nature, to all ordered life. The extraordinary proliferation of "monasteries" within this society encompassed many foundations that were



closer to a modern kibbutz than to the regulated splendor of contemporary Benedictine St. Gall or Cluny. One hopes that Linage will explore, in another work, the social and economic bases for the spread of Spanish monasticism. It is clearly linked with the privileged legal independence enjoyed by any "monastery." The connection is made here with the peculiarly Spanish institution of the monastic pact, which—unlike Benedict and other monastic legislators—limited the monarchical powers of the abbot by the bilateral contract it established between him and his monks. Spanish pactualism, born in seventh-century Galicia, proved so well suited to the settlement of frontier lands in the slow advance southward that, despite its flagrant defiance of all monastic norms, it survived until about 1050. With some differences of detail Linage follows here the studies of Professor C. J. Bishko, as against the recent views of Dom Justo Pérez de Urbel. (See now Bishko, "Hispanic Monastic Pactualism: The Controversy Continues," *Classical Folia*, 27 [1973]: 173–85.) Linage suggests a parallel between pact and *fuero*. Spanish frontier society, whether in monastery or municipality, needed a framework to contain its tensions, a precise statement of rights and obligations.

In tracing the slow infiltration of the Benedictine rule into a fragmented frontier Linage uses two main types of evidence, monastic documents and literary works. In the documents there is no evidence for formal Benedictine observance before 1028 in Aragon, before 1032 in Navarre, before 1042 in Asturias, and before 1077 in Galicia. The diffusion of Benedictine influence can be traced, however, not only through manuscripts of the rule itself but through the commentaries of Smaragdus and the *Dialogues* of Gregory the Great. These works spread before 900 from Catalonia and southern France to Castile. Within a century they were influencing Castilian monastic legislation. The picture remains inchoate, however, until the coming of Cluny, whose commanding role Linage revindicates against recent questioning. Cluniac Benedictine monasticism penetrated through Navarre into Castile, León, and finally, Galicia and Portugal. It promoted the general eleventh-century Europeanization—in spirituality, culture, the organization of Church and State—of the western kingdoms of the peninsula. The time lag of three centuries between Castile and Catalonia proved, however, too considerable to bridge.

Volume 3 of this remarkable work is a "Monasticon Hispanum," which contains a

catalog of all the monasteries the author can document (31 from 398 to 711, 1,828 from 711 to 1109). Although this catalog does not pretend to be exhaustive, it is by far the most complete yet published and the basis for the definitive work we need. It is unfortunate that the catalog (and the map in vol. 2, p. 867) deliberately omits the Catalan monasteries under the influence or control of Abbot Oliba that are at present in French territory.

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ANWAR G. CHEJNE. *Muslim Spain: Its History and Culture*. Minneapolis: University of Minnesota Press. 1974. Pp. xvi, 559. \$24.75.

Professor Chejne's book is a useful addition to the growing number of works in English dealing with medieval Spain. It will serve as a handy reference for many aspects of the history and culture of Muslim Spain. The first five chapters review political history from the conquest in 711 to the fall of Granada in 1492. The remaining sixteen chapters are devoted to such themes as society, government, literature, historiography, religious and philosophical studies, science, art, and architecture.

While the book contains considerable information, it has several weaknesses. Most chapters range over the entire seven-hundred-year period and do not give sufficient weight to the changing conditions of the several historical epochs of Muslim domination. Moreover, while properly emphasizing Muslim contributions to Christian Europe, the author adopts an excessively defensive attitude that seems out of date, and his explanation of Christian cultural developments is overly simplistic. In castigating Alfonso X for not having shown greater appreciation of the Muslim cultural contribution, he demands more than one can reasonably expect from a thirteenth-century Christian king, just as, conversely, one would not expect ibn-Hazm to wax rapturous over Christian theology or philosophy. The chapters on political history skim through the Granadan period and neglect the development of Christian Spain; no attempt is made to explain Muslim policy toward the Christian north. The author implies that Christians and Muslims lived in harmony until Alfonso VI was carried away by an excess of religious zeal in the eleventh century. It is strange that little attention is given to Almanzor's nearly annual expeditions against the Christians in the tenth century.

Professor Chejne offers no new interpreta-

tions and is often repetitious in stating facts or ideas. His book is well documented and is supplied with an ample bibliography, especially valuable for its listing of Arabic manuscripts and printed sources. There are several maps, though the island of Minorca is missing on them. Several of the dates assigned to Christian rulers are incorrect. For all its deficiencies, this book is helpful and will no doubt achieve one of the author's principal aims, to encourage further study of Muslim Spain.

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HANS GEORG REUTER. *Die Lehre vom Ritterstand: Zum Ritterbegriff in Historiographie und Dichtung vom 11. bis zum 13. Jahrhundert.* (Neue Wirtschaftsgeschichte, number 4.) Cologne: Böhlau Verlag. 1971. Pp. ii, 207. DM 24.

This essay undertakes a very necessary work of demolition, and its success reminds us of the truth that for a better understanding of the past we do not so much need new evidence as a more rigorous examination of what evidence we already have. The author asks whether the literary historians' belief in the existence of a knightly class, which was responsible for the lay culture of the German High Middle Ages, can be sustained by contemporary literary and historiographical evidence and concludes that it cannot. His method is to interpret the key words "Ritter" and "miles" in all kinds of context and to demonstrate thereby that the variety and fluidity of their meanings make it impossible to apply them as fixed class or functional labels. Even the often cited tripartite division of medieval society—clergy, warriors, and farmers—is shown to be less contemporary sociology than hortatory exegesis. If there was an ordering principle, and it is not certain anyone felt a need for one, it classified people according to the powers they exercised over land and other men. Modern attempts to accommodate the tangle of reciprocal rights and duties these powers entailed to the tidy nineteenth-century concepts of "class" and "profession" have compounded the errors made first by eighteenth-century legal historians and then by Romantic literary critics, who saw courtly literature as a true reflection of medieval life and its knights as a unified class self-consciously expressing the spirit of their times. With the poetry thus misunderstood by its most authoritative interpreters and thereafter appealed to as historical evidence, fiction has been piled on fiction. The author's

scrutiny of the literature produced in Germany between 1060 and 1250 provides such clear evidence of the falsity of the received view that one can only wonder why it has been clung to for so long.

BERNARD S. SMITH  
Swarthmore College

REINHOLD SCHUMANN. *Authority and the Commune: Parma 833–1133.* (Fonti e studi, Second Series, 8.) [Parma:] Presso della Deputazione di Storia Patria per le Province Parmensi. 1973. Pp. xxi, 397, 12 maps. \$19.00.

How, in Parma and similarly in other towns, did the civic assembly, around the turn of the eleventh century, acquire authority that had at one time been exercised from above, when the assembly itself had been formed from below? A good discussion of a question like this one has bearing on every study of government in the Middle Ages and later, and a good discussion means a full discussion, which is what Schumann has given us.

The scope of the book, as its title indicates, extends far beyond the moment a broad segment of the population takes on a significant part in legitimate government. There is, first, a close examination of comital authority under the later Carolingians, not as simple a matter as used to be imagined, and one Schumann can illuminate with material from the Lombard and late Roman periods. The greater part of the study, and the most complex, takes up what happened to this comital jurisdiction, its distribution among families of various kinds and, largely, to the church of Parma in its various manifestations. The account goes beyond the acquisition of authority by the civic assembly and others to conclude with the establishment of the commune in 1133, all in a well-developed context of reform and papal-imperial rivalry.

The central virtue of the study is the care its author has taken to work up each point in his argument with necessary material. He mines his documents for indications of the interests represented in them and the norms and expectations that, if understood, constitute their meaning. Its central vice is, naturally, the monographic style, but it is not unreadable, often insightful, and always clear when read with care. In his analyses Schumann considers evidence from topography, iconography, genealogy, political circumstance, and just about everything else available to supplement his own command of the diplomatic. Most valuable, I

think, is his careful derivation of juridical and political relationships among various categories of the population, always with regard to the interests and practical needs of individuals and groups, along with his specification of the circumstances that led higher authorities to make the concessions they did. There is a good anchor here for future speculative discussion of this period, of which there has been, in recent decades, so much in comparison to the amount of real history written. A full apparatus is provided, more useful than elegant; the comments in the bibliography complement Schumann's restraint regarding polemic in text and notes.

AVERY ANDREWS

George Washington University

J. K. HYDE. *Society and Politics in Medieval Italy: The Evolution of the Civil Life, 1000-1350*. (New Studies in Medieval History.) New York: St. Martin's Press. 1973. Pp. xv, 229. Cloth \$13.95, paper \$4.95.

Traditional interpretations of the Middle Ages have consistently failed to make much sense of Italian history. Manorialism, feudalism, national monarchies, Scholasticism, and the Gothic style—the characteristic themes that fill our textbooks—are, in Professor Hyde's just assessment, "generalizations of certain features found chiefly in parts of England, northern France and western Germany." They are further "quite atypical of most of Italy." But Italian medieval civilization cannot be dismissed as a "local deviation from the norm." Italians—merchants, papal legates, lawyers, and teachers, to mention only the most prominent—helped shape and cement the international culture of the Middle Ages. Hyde recommends that English-speaking medievalists must now find room to include, alongside their discussions of feudalism and the manor, an examination of the dominant trait of medieval Italian society—its civil life.

In a brief 200 pages, Hyde examines cities and civil life in Italy from the tenth to the fourteenth centuries. This is Italy viewed between two crises—the gloom and troubles of the tenth century and the catastrophes of the fourteenth. Her trade then extended through Europe and the Levant, and her growing cities teemed with spontaneous associations of every sort—consorteries, guilds, confraternities, parties, and the commune itself. Hyde describes the economic foundations of this vigorous urban growth; he also studies these urban associations, within which the civil life was largely lived. In broad strokes he traces the institutional and

political development of the medieval communes. The complex history of the medieval Italian town is without doubt a morass in which many skilled authors have lost their readers. But Hyde maintains a firm sense of direction and proves to be a sure guide. He sharpens his interpretations and enlivens his text with frequent allusions to original records. And he provides a short, annotated, eminently useful bibliography of works in English and foreign languages, for those who wish to explore further.

Treating complex themes in compressed space, the book might occasionally be faulted for misplaced emphases. Hyde argues that the unity of medieval Italy was fundamentally cultural, but the reader is told little about culture. The only intellectual figure to receive close attention is Marsiglio of Padua; the discussion of Marsiglio closes the book, and this unfortunately suggests that he, rather than Dante or Giotto, provides the culminating cultural expression of medieval Italian civilization. But apart from questions of balance, the book remains a marvel of concise, clear, and accurate presentation of economic, institutional, and political history. Hyde affirms that "without some general picture of medieval Italy, neither the Middle Ages nor Italy can be properly understood." For the period of his principal concern, he has provided probably the best short, general picture of medieval Italian urban history that exists in English.

DAVID HERLIHY

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MICHAEL MALLETT. *Mercenaries and Their Masters: Warfare in Renaissance Italy*. Totowa, N.J.: Rowman and Littlefield. 1974. Pp. 284. \$15.00.

Many misconceptions about the nature of Renaissance Italian warfare are traceable to contemporary observers who blamed the mercenary system for defeats suffered at the hands of foreign arms. Machiavelli, in particular, converted his admiration for the citizen armies of Rome into an analysis of the military vices of Italy. With the help of some admittedly egregious examples, tradition has perpetuated the image of the *condottiere* as a colorful, but faithless, violent, and unprincipled mercenary whose crowning vice was incompetence.

Dr. Mallett's work comes as the most recent accession to a body of scholarship that has begun to contest this view. Building on the work of students of military history such as

Piero Pieri and John Hale, Mallett has marshalled the sources to present the *condottieri* as on the whole reliable professionals whose mastery of their craft was by no means inferior to that of northern captains, and whose style of warfare was dictated by political and economic considerations of the states they served. That battles were bloodless turns out to be a demonstrable fiction. If engagements were often inconclusive, and warfare a science of attrition rather than of decisive encounters, this was owing to the policies of the states themselves.

Whereas in the late *trecento* and early *quattrocento* the *condottiere* with political ambitions was a frequent phenomenon, toward the middle of the *quattrocento* he became a full-time employee, closely bound to his employers. Of all the Italian states, the most backward in military matters was Florence, whose distrust of mercenaries proved self-fulfilling. Because of their avoidance of long-term contracts, the Florentines tended to be badly served by short-term *condottieri* with weaker loyalties. Once again the dangers are shown of viewing Italian history solely through Florentine eyes.

In treating Italian military affairs as a function of the political and economic organization of the Italian states, Mallett follows the pioneering work of Pieri. His own researches, however, have made possible greater clarity and precision, and he has gone on to explore the social dimensions of the mercenary system. His study does not always avoid the pitfalls of revisionism: some chapters read like a running dialogue with Machiavelli. Nonetheless, to have brought a new degree of order to a vast, complex, and important subject is a notable achievement.

T. C. PRICE ZIMMERMANN  
Reed College

MICHEL BALARD. *Gênes et l'outre-mer*. Volume 1, *Les actes de Caffa du notaire Lamberto di Sambuceto, 1289-1290*. (École Pratique des Hautes Études—Sorbonne, VI<sup>e</sup> Section. Documents et recherches sur l'économie des pays byzantins, islamiques et slaves et leurs relations commerciales au Moyen Âge, 12.) Paris: Mouton & Co. 1973. Pp. 420. 72 fr.

This volume containing the notarial acts redacted in the Genoese colony of Caffa by the notary Lamberto di Sambuceto between April 24, 1289, and August 17, 1290, is a welcome addition to the small but important corpus of published medieval notarial materials. Professor Balard has brought together and ordered

chronologically 903 of Lamberto's acts from the various cartularies and bundles into which they had been haphazardly placed following the shelling of the Genoese archive by a French fleet in 1684. The material complements the notarial acts from Pera and Caffa published by G. I. Bratianu (*Actes des notaires génois de Pera et de Caffa de la fin du XIII<sup>e</sup> siècle 1281-1290*) in 1927.

The contracts themselves reflect both the long-range trade and commerce for which Caffa was an entrepôt but shed light as well upon the workings of the local society and economy. *Commenda*-type commercial associations are reflected in 133 contracts while the *societas maris* is exemplified in only six. The preference for the unilateral *commenda* over the bilateral *societas maris*, which follows the pattern of both Genoa and Venice, may well have been the result of the easy availability of venture capital. Thus the traveling merchant could attract capital without committing his own resources. Sixty-seven exchange contracts illuminate the mechanism for transferring funds, primarily between Caffa-Pera and Caffa-Genoa, in support of commerce or to effect returns of capital. Interestingly the editor is able to calculate the range of profit that a merchant-banker might expect on exchange and re-exchange transactions as falling between 3.5 per cent and 20.6 per cent depending upon the fluctuations of the exchange rates.

Other types of commercial activities reflected in the documents include the leasing or buying of ships, purchase-sale contracts, quittances, sea loans, and straight loans. Caffa's importance as a center of the slave trade is dramatically demonstrated through sixty-some contracts. Commerce in raw silk, one of the staples of Genoese commerce with the East, is, however, strangely underrepresented and is mentioned in but six contracts.

The local economy emerges in numerous contracts for the sale of commodities for consumption, mainly wine, to local artisans. Marriages, dowries, and wills also appear among Lamberto's minutes.

In short these skillfully edited contracts are typical of medieval notarial materials in general in the rich variety of information they furnish about the day-to-day economic and social life of medieval society—in this case a unique Western commercial outpost in the Crimea.

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S. D. GOITEIN, translated from the Arabic with introduction and notes by. *Letters of Medieval Jewish Traders*. Princeton: Princeton University Press, 1973. Pp. xviii, 359. \$12.50.

The Cairo genizah—this unique storehouse of medieval Jewish documents, all of which contained or might have contained the name of God in Hebrew—has yielded up some twelve hundred more-or-less complete business letters and accounts. Eighty of these letters were selected by the author, a member of the Institute for Advanced Study, Princeton, to illustrate the mercantile world of the medieval society in the Arabic Mediterranean. From that world and that period only the letters and other papers of Jewish overseas traders have thus far been found, although Middle Eastern Christians as well as Muslims observed the same pious custom of preservation.

More than half of the documents presented are from the eleventh century, twenty-five from the twelfth century, and the balance from the early thirteenth century. The letters originated in practically all Islamic countries from Spain and Morocco in the West to India in the East. There are frequent references to Sicily, under Muslim and Norman rule for most of this period, and some to Christian Italy—trade then as now knows few religious or political limitations. The language of this correspondence is vernacular Arabic in the Hebrew alphabet except for one Arabic letter addressed to a Muslim judge and a Hebrew letter written by an Italian Jew.

The subjective aspects of international trade are stressed in this collection: because of the general insecurity and the slowness of communications, personal relationships and mutual confidence—which then usually meant a relative or a member of one's own religious community—formed the basis of business practices. God is constantly invoked in these letters, not as a mere *façon de parler*, but as the only true support and guide against whims of nature, the menaces of piracy, war, and arbitrary government.

A great deal of the socioeconomic history of the Middle Ages can be reconstructed from these genizah letters, types of merchandise exchanged, flow of specie from West to East, patterns of sea traffic, and so on. Goitein's book, even though experimental, represents a significant contribution to the study of history, economics, religion, and social relations. It is not easy reading, especially since each one of the numerous footnotes requires careful attention. The immense scholarship, profound wisdom,

and deep humanity of the author is evident on every page of this work.

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N. ADONTS. *Armenia v epokhu Iustiniana* [Armenia in the Time of Justinian]. (Erevanskii Gosudarstvennyi Universitet, Tsentr Armenovedcheskikh Issledovanii.) 2d ed.; Erevan: Izdatel'stvo Erevanskogo Universiteta, 1971. Pp. 526.

The original edition of Adonts's book appeared in 1908 as a methodologically innovative product of his research activities at the University of St. Petersburg. Although the main emphasis of his work concentrated on the impact of Justinian's reforms on the political, church, and social developments of West Armenia, its scope exceeded the chronological framework suggested by the title. A thorough survey of diversified geopolitical components of ancient and medieval Armenia, various toponymic and ethnographic observations, an inquiry into historical processes in the centuries prior to the advent of Justinian and into manifold consequences of the Byzantine and Persian policies for Armenian society in the sixth and subsequent centuries, and, especially, a rigorous analysis of the origin, structure, and socioeconomic implications of the institution of the *nakharardom*—all of these studies based on a vast array of meticulously interpreted and re-interpreted multilingual source materials and scholarly literature—have established Adonts's contribution as a pioneering work serving as a mine of important information and as an inspiring guide stimulating progress in the field of Armenian history.

The best tribute to the significance of Adonts's initial accomplishment has been rendered by the appearance of two new editions of his book some sixty years after its original composition. In 1970 Professor Nina Garsoïan published a revised, bibliographically enriched, and methodologically updated version in English (*Armenia in the Period of Justinian*), making the contents of Adonts's contribution accessible to readers unfamiliar with the Russian language (in this case especially difficult since it was not the natural medium of communication of the Armenian scholar). She also provided her edition with a critical introduction evaluating the fundamental merits and some of the limitations of Adonts's work, in the light of modern Armenological studies. The Center of Armenian Studies of the State Uni-

versity of Erivan placed *Armenia v epokhu Iustiniana* at the disposal of readers in the Soviet Union, in spite of its pre-Marxist historiographical character. A lengthy introduction by K. N. Iuzbashian sketches Adonts's biography, retraces the genesis of his book, and discusses its impact on Armenian studies. Unlike Garsoïan's revised, updated, and scholarly enriched re-edition, the Erivan publication does not furnish any bibliographical or revisional supplements (except for a footnote on page 19 listing a number of Russian contributions), thus depriving Russian scholars of important information regarding the progress achieved in the West in this fascinating field of history.

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ANITRA GADOLIN. *A Theory of History and Society, with Special Reference to the Chronographia of Michael Psellus; 11th Century Byzantium*. (Acta Universitatis Stockholmiensis. Stockholm Studies in History of Literature, 11.) Stockholm: Almqvist & Wiksell. 1970. Pp. 217. 35 S. kr.

This is a close, independent, and careful literary analysis of the *Chronographia* of the eleventh-century Byzantine statesman and intellectual Michael Psellus. His history is one of the three or four best classics of Byzantine historiography. Gadolin's analysis is useful for both Byzantinists and nonspecialists; yet its organizational structure is unnecessarily complicated, making it difficult to read and to use. Gadolin demonstrates very convincingly that one must study the *Chronographia* in relation to classical Greek rhetorical and literary models. Yet there is no attempt in this monograph to relate the *Chronographia* to traditions of classical Greek and Byzantine historiography—a serious flaw. The author has collected from Psellus a vast group of references on neglected topics. Such references should make the work indispensable for analysts of eleventh-century internal Byzantine politics. A major conclusion (pp. 56–57) is that Psellus, contrary to the hitherto most prevalent scholarly opinion, did not show favor to the civil element in the government. Indeed, he stressed the key role of the military. This point deserves a more rigorous examination using all relevant sources and modern scholarly literature. It appears that we also should re-examine eleventh-century internal factionalism. One needs to remember that

bureaucrats such as Psellus and the Caesar John Doukas (p. 80) evinced interest in military tactics and strategy. Gadolin rightly points to the Byzantine Empire's dependence for survival upon the scrupulous observance of an understood system of honors and pecuniary rewards together with their prudent distribution (p. 153). This topic also awaits a thorough historical investigation.

Gadolin's book should have some appeal to those historians interested in comparative studies—as well as Byzantinists—because it raises, although it does not solve, wide-ranging questions concerning a broad spectrum of topics including the role of women, castration, eunuchs, Byzantine esthetic ideals, and style preferences. A number of technical deficiencies mar the book. It lacks an index. The bibliography is hopelessly out of date (not surprising when one relies upon the 1942 edition of G. Moravcsik's *Byzantinoturcica!*); Lebeau is, for example, cited all too frequently. Most important, the author has failed to relate the literary analysis to contemporary and past historical context. The book is useful, yet should be handled with caution. Psellus is again receiving scholarly attention. For a different treatment of Psellus, including the edition of many new texts, compare Günter Weiss, *Oströmische Beamte im Spiegel der Schriften des Michael Psellos* (1973), who does not cite Gadolin's work. Gadolin's book marks a substantial advance, but the *Chronographia* of Michael Psellus deserves more thorough historiographical analysis.

WALTER EMIL KAEGLI, JR.  
University of Chicago

#### MODERN EUROPE

DEREK JARRETT. *The Begetters of Revolution: England's Involvement with France, 1759–1789*. Totowa, N.J.: Rowman and Littlefield. 1973. Pp. xiv, 320. \$13.50.

Professor Derek Jarrett presents an interesting and provocative thesis in his latest book. Although his thesis may not be entirely convincing, it should stimulate fruitful debate among scholars and students of the Atlantic Revolution of the eighteenth century.

Jarrett argues that in addition to the well-known diplomatic and economic relations between England and France, there also existed a complex but pervasive intellectual and political involvement between the two states. Intelligent men in both countries feared, ad-



mired, and reacted to their opposite numbers. The English feared French Catholicism and absolutism while admiring France's order and stability. The French feared British commercialism while admiring England's dynamic social and economic system. In both lands, certain politicians tried to represent the country above factions and aristocratic cliques. Other leaders through bureaucratic reforms attempted to impose efficient rational methods on the operations of government. Finally, the author concludes that this relationship was at least partially productive of the Revolution of 1789.

Jarrett's view that politics in England and France between 1759 and 1789 were too closely interwoven for the history of either country to be intelligible on its own is an appealing and even a plausible idea. Certainly it is an idea worthy of close examination. I do not feel, however, that the author conclusively demonstrates that these interrelationships sustain the conclusion stated in the title. Moreover, many of the parallels that he draws seem too facile to warrant acceptance without additional examination. To view Pitt the Elder and Choiseul, Necker and North, and Calonne and Pitt the younger as pursuing virtually identical policies seems to ignore significant, even critical, differences among them.

During the eighteenth century many states faced similar social, economic, and political problems. Solutions and attempted solutions differed widely and produced a wide variety of results. Perhaps differences rather than similarities among states are the decisive factor. Nevertheless, Jarrett has produced an intelligent, stimulating, and inventive approach to the problem of French and British political development. It is an approach meriting careful attention.

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Newport

NEVILLE WAITES, editor. *Troubled Neighbours: Franco-British Relations in the Twentieth Century*. (Reading University Studies on Contemporary Europe.) London: Weidenfeld and Nicolson. 1971. Pp. 386. £4.20.

This collection of lucid, well-written, well-researched essays covers the many vicissitudes and the occasional high points of the Anglo-French relationship since 1900. The ten British and three French authors almost all manage to give full and fair treatment to both sides, making use of much recently available material,

often offering new interpretations, and most of them providing very helpful and thorough bibliographical studies at the end of each chapter. The essays cover the major diplomatic developments and events: the Entente Cordiale from its origins to 1914 (Christopher Andrew); strategic and economic relations during the First World War (J.-B. Duroselle); the making of the Versailles Treaty (D. R. Watson); Locarno (Douglas Johnson); the depression years (Neville Waites, who also contributes a stimulating introduction); the Rhineland crisis (Maurice Baumont); reactions to Munich (Anthony Adamthwaite); the breakdown of the alliance in 1940 (P. M. H. Bell); the second wartime alliance (Keith Sainsbury); the reconstruction and defense of Western Europe after 1945 (Geoffrey Warner); the Allies at Suez (Hugh Thomas); security and settlement in the Mediterranean since 1914 (Ann Williams); and the defense and unity of Western Europe since 1958 (Guy de Carmoy).

Despite its many virtues, the whole effect of the book, however, leaves one faintly dissatisfied. What emerges from these pages is not so much a picture of Allied cooperation as a record of misunderstanding, suspicion, false perceptions, and mutual frustration—and there is little attempt to penetrate behind the outward ups and downs of diplomatic relations to illuminate the more basic question of why this was so. On one hand, the book would have profited greatly from a much more extended treatment of the pervasive influence of economic relationships. On the other hand, the whole area of images, attitudes, perceptions, and assumptions each nation had about itself and its neighbor—in some ways speculative but perhaps more fruitful than the narrower diplomatic history to which most of the essays restrict themselves—is often implied but seldom directly discussed by authors who cannot explain the continual friction and misunderstanding as a simple divergence of national interests. (John Cairns's "A Nation of Shopkeepers in Search of a Suitable France: 1919-40," *AHR*, 79 [1974]: 710-43, is precisely the sort of thing this volume needs.)

The three essays on the 1930s illustrate the book's strengths and weaknesses. Neville Waites provides a generally sound account of the tangled story of the depression years. But even the new sources he has used offer an opportunity to penetrate more deeply into the problems of British policy in this period, to the level of the state of mind, the assumptions, behind specific policy decisions. The papers of

Sir John Simon, for instance, reveal throughout the government's preoccupation with its standing with the public and the intense, continual public pressure for at least the appearance of a success in "collective security" and disarmament. At the same time, the memories of 1914 were very strong (indeed, few of the essays provide much continuity with the historical legacy of earlier periods): in July 1934, after the Austrian putsch, Simon wrote to MacDonald, "We must keep out of trouble in Central Europe at all costs. . . . July twenty years ago stands as an awful warning. . . ." These preoccupations, including the fear of alliances and an arms race that would simply bring on a repetition of 1914, led Simon and other British policy makers to the gloomy conclusion that there was very little they could do to resist German encroachments, whether in armaments or elsewhere—let alone take forcible measures to restrain Germany. Far from there being a "transition in British policy towards appeasement from late 1933" (p. 127), there was a basic continuity in policy from 1931 on, with clear parallels between, for instance, the Manchurian and the German problems.

The fruits of the Anglo-French predicament were harvested first in the Rhineland crisis in 1936, and it is a pity that Baumont's chapter is the shortest and the least useful in the volume. But Munich was a later and more bitter fruit, whose treatment here by Anthony Adamthwaite covers not only the course of policy, especially French, with a fine detailed picture of Daladier, but also the whole set of attitudes behind French appeasement generally, from the "mood of national self-doubt and pessimism" to the franc as "the Achilles heel of French policy" (p. 178). It is one of the best essays in a long and depressing story of lost opportunities between two great nations who could neither live with nor survive without each other.

EDWARD B. SEGEL  
*Reed College*

PAUL C. HELMREICH. *From Paris to Sèvres: The Partition of the Ottoman Empire at the Peace Conference of 1919-1920*. Columbus: Ohio State University Press. 1974. Pp. xiii, 376. \$15.00.

The Peace Conference of 1919-20 was no ordinary one in which the victors simply confirmed a military outcome. Rather, it was the arena in which were played out a multitude of political, economic, social, humanitarian, and personal threads whose results were to influence the

course of Near Eastern affairs to this day. In disposing of the defeated Ottoman Empire the Allied powers were faced with the need to create an entirely new political structure, an enterprise that eventually became little more than a struggle among the victors to advance their own particular interests in the area. Britain, France, Italy, and for a time the United States also found themselves faced with the need to adjudicate rival claims from a long line of parties from the former Turkish territories who appealed to a diverse and often conflicting array of moral doctrines (e.g., Armenian self-determination), multiple political promises (such as those of Britain to Emir Faisal), and area rivalries (like that between the sultan's government in Istanbul and Mustafa Kemal's Turkish nationalists in Anatolia). Here also were some of the most forceful personalities of the era, including Lloyd George, Clemenceau, and the often masterful Greek prime minister, Venizelos—men, Helmreich shows, who were able to persuade their fellow negotiators by the force of their own personalities and diplomatic skills, yet who on numerous occasions were also forced to change their positions in response to domestic political developments.

In the end, as is so often the case, the outcomes were primarily influenced by the realities of power, resources, and "sudden worthlessness of solemn promises" as soon as "no national, commercial, or imperial interest could be served." Thus the often promised independent Armenian state evaporated because of the unwillingness of the powers to commit the forces needed to defend it until it could become viable on its own; Woodrow Wilson's entreaties on many matters were ignored as soon as it became apparent that the United States could not be counted on to help enforce any of the agreements; and what was perhaps the most important decision of the conferences, encouraging Greece to pursue its interests in Asia Minor by military force, was quickly to be undermined by the all-but-collapse of the sultan's government and by the separate withdrawals from Asia Minor of the French and Italians, all of these things adding encouragement to the Turkish nationalist armies whose role the powers had very seriously underestimated.

Helmreich has cut through these tangled webs with great skill and in a well-organized fashion that makes it almost amazingly easy to see the evolution of particular issues and to keep track of the many strands and dramas.

Among his conclusions, perhaps the most significant is that "the mighty labors of a year and a half brought forth a stillborn treaty" because, among other factors, "the treaty was negotiated to settle the prewar Eastern question by men who saw the problem only as it had existed before the war, . . . and the delay of close to two years [after the armistice] was fatal for it allowed a whole new set of problems and circumstances to develop in the Near East"; on the other hand, "the Treaty of Sevres was drawn up too soon for the negotiators to be able to fully assess the meaning of these new forces." The importance of these points in an era of so unusually great and rapid change as characterized these particular years is rightly stressed by Helmreich, and he might well have added that similar problems beset many, if not most, peace conferences in the modern world. Helmreich makes many other assessments of policies, governments, and the individual negotiators, and this reviewer would disagree with some of the latter, but this is only a very minor criticism of a book that should be seen by students of diplomacy and international relations, those interested in the Middle East, and current practitioners of the diplomatic art. The lessons to be learned from the Peace Conference of 1919-20 about promises, commitments, domestic politics, personalities, and power are plentiful indeed.

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MORTIMER LEVINE. *Tudor Dynastic Problems, 1460-1571*. (Historical Problems: Studies and Documents, 21.) New York: Barnes and Noble. 1973. Pp. 191. \$9.50.

Professor Levine, probably this country's leading student of Tudor dynastic history, has provided us with a valuable and convenient reference. A clear path is cut through the tangled growths surrounding questions of title to the throne from the time of Henry VI to that of Elizabeth I. There are few surprises here: the essay usually follows well-established interpretations in explaining why the Crown descended as it did and in assessing the constitutional issues implied in the many royal attempts to channel the succession. Each sovereign tried to name his successor with such authority that his will would bind the nation even in death, and some, notably Henry VIII and Edward VI, came close to creating instruments of absolutism in their attempts. But the final outcome, em-

bodied in Elizabeth's Second Treason Act of 1571, was "virtually" to establish parliamentary statute as the ultimate arbiter of succession questions.

The constitutional issue was of greater consequence, but the plottings and schemings of kings and aspirants to kingship have given the succession issue its enduring popular appeal. Levine sorts out the problems and personalities with precision; it is in discussion of these matters that the reader finds an impression of the crowned heads as persons and as administrators. Henry VII, for example, is seen as humane as well as scrupulously constitutional; he treated rivals with as much generosity as his safety could possibly allow and recoiled from sentences of death. Here as in other recent studies he seems more and more to resemble his granddaughter and not his son. Henry VIII, too, followed the letter of the law—his care in gaining parliamentary sanction for his changes in the order of succession laid most of the foundation for Elizabeth's treason act—but he had little regard for the lives of even potential challengers. His destruction of Buckingham and the Pole family was not justified by any identifiable treasons on their part and were acts of despotism. But it was Edward VI—or Northumberland acting for him—who went beyond despotism to tyranny; the device for placing Jane Grey on the throne in effect commanded treason from the whole country. Levine rejects W. K. Jordan's thesis that it was Edward himself who engineered the scheme and returns to the older view that the boy-king was a pliant vehicle of the grasping minister.

The essay on succession, although billed as an introduction to the documents, takes up most of the volume; the documents become merely an appendix, mostly drawn from well-known sources, which substantiates the argument. Even so, the text is cut short; only a couple of pages form a kind of epilogue for the years 1571 to 1603. According to Levine, the problem was settled by the Second Treason Act, but one's sense of consistency requires some attention to Mary Stuart, the negotiations with James VI, and the fates of Elizabeth's remaining cousins.

ROBERT W. KENNY  
George Washington University

WHITNEY R. D. JONES. *The Mid-Tudor Crisis, 1539-1563*. New York: Barnes and Noble. 1973. Pp. 226. \$13.00.

This book is at once an outgrowth of, and a

supplement to, Dr. Jones's earlier monograph, *The Tudor Commonwealth, 1529-1559*, in which the full impact of social and economic developments during a thirty-year period of crucial upheaval was examined. Although the present study extends the terminal limits of the crisis period and widens the scope of inquiry, it deals essentially with the same problems. The attempt, however, to justify a new "crisis"—not crises—for the years 1539 to 1563 is unconvincing.

The term "crisis," like "transition" and "revolution," is at best relative, with no precise meaning, and, hence, should be used sparingly; when applied to a particular date or to a historical division of time, it is often misleading. Undoubtedly, the period under review was replete with a succession of problems and danger signals approaching crisis proportions for the government, but to describe the entire quarter of a century as a crisis unit is an oversimplification. This could easily have been avoided by a straightforward interpretation under the title, "Tudor Crises, 1539-1563," without detracting from the validity of the study.

This work is not a textbook; neither is it a survey or a research monograph. Rather, it is, in the words of the author, "an introduction to recent studies of important aspects of the mid-Tudor crises," suitable for undergraduates and upper sixth form students who have some previous knowledge of the subject (preface). As a synthesis of a very important period in Tudor history, it traces in successive chapters the crises in governance, religion, foreign affairs, and in social and economic development, with particular emphasis upon different points of view as found in modern historical accounts. All the controversies are analyzed with precision and insight, and, while not resolved, they are objectively set forth with discernment and clarity. In this respect it is an admirable study.

*Mid-Tudor Crisis* is well written, with a good select bibliography and index; the conclusion is sound. Unfortunately the excellence of presentation is marred by the apparent necessity of grouping notes together in order to save space, a device that frequently renders the precise identification of quotations and references virtually impossible.

W. C. RICHARDSON  
Louisiana State University

IRVIN BUCKWALTER HORST. *The Radical Brethren: Anabaptism and the English Reformation to 1558*. (Bibliotheca Humanistica & Reformatoria,

volume 2.) Nieuwkoop: B. de Graaf. 1972. Pp. 211. 65 gls.

This study is based on fragmentary references, mostly from hostile sources, to the presence of Anabaptists in England. The author acknowledges that "anabaptist" supplanted "Lollard" after 1530 as a descriptive epithet for almost all expressions of English nonconformity. It is clear that Anabaptists from the Low Countries were present in England as early as the 1530s, that they established contact with surviving elements of Lollardy, and that a number of Anabaptist books in English were circulated during the latter years of Henry VIII. It is also clear that these Anabaptist influences merged with and were reshaped by the earlier Lollardy. Horst, in an effort to identify an English "anabaptist" movement, casts his net wide and includes within the movement almost every expression of lay piety that was tinged with one or more Anabaptist doctrines. Even a rejection of predestination and an assertion of free will seems sufficient to bring an individual or a group into the fold, although a denial that Christ took on sinful flesh from the Virgin is an added bonus for purposes of identification. Indeed, the "robust radicalism" that the author depicts is broad enough to embrace most of the lay nonconformist piety, which was humanist in orientation and stemmed from Erasmian influences.

The author concludes that "the principal distinction between anabaptism in England and the larger movement on the Continent" is "that English anabaptism was not separatist and did not institute rebaptism." If this is true, one wonders why, then, should the label "anabaptist" be pinned on the movement? Indeed, one wonders why manifestations of dissent that are so varied and diverse should be called a movement.

WINTHROP S. HUDSON  
University of Rochester

ALAN EVERITT, editor. *Perspectives in English Urban History*. New York: Barnes and Noble. 1973. Pp. 271. \$16.50.

The essays gathered in this book cover such disparate and unrelated topics—the late medieval town council at Stamford, trade patterns in Tudor York, the houses of Burford, English inns from 1560 to 1760, prerailway Margate, schooling in early Victorian Coventry, urban renewal in Victorian Croydon, and village carriers in Victorian Leicestershire—that at first glance one



might wonder whether they would not have been more appropriately published separately in the scholarly journals. But the book is more than an *ad hoc* collection, for uniting these informative essays is a common commitment, amounting almost to pioneering fervor, to the comparative analysis of local urban institutions. Although the sometimes too intrusive research techniques are by no means as innovative as several authors claim (indeed, the methodology often resembles that of the *Victoria County Histories*), the entire tone of the book is fresh and invigorating. Firmly in focus is the relationship between a town and its immediate economic hinterland and the "pronounced introversion of the urban organism" of the small market town and the importance of these to an understanding of the dynamics of urban development.

It is perhaps invidious in a review of this size to single out essays. All are scholarly, but three in particular transcend their local setting to offer fruitful suggestions for comparative urban analysis. Professor Everitt's study of the urban inn thoroughly demonstrates how that splendid institution served as public lecture hall, corn exchange, auction and ball room, even bank and town hall, before succumbing, like so much else, to the railways and Victorian specialization of function. Mr. Laithwaite demonstrates how the study of domestic topography (in Burford) may evoke and illuminate the social fluctuations of a community, and Mr. Stephens develops the interesting thesis that educational standards and expectations among the domestic textile workers of Coventry were lower than those in factory towns—a conclusion that deserves close examination by social historians.

This book persuasively calls for balance in English urban studies. As Everitt comments, Victorian England had its Melton Mowbrays as well as its Manchesters, and perhaps almost one half of the population in 1900 still lived under the influence of the small market town, so celebrated in English novels, yet so sadly neglected by historians. Graduate students searching for likely dissertation topics will find this book a gold mine and will be aided by the annotated bibliographies to each chapter, which more than atone for the unfortunately sparse footnotes.

ANTHONY S. WOHL  
Vassar College

MAURICE LEE, JR., edited with an introduction by *Dudley Carleton to John Chamberlain, 1603–*

*1624: Jacobean Letters*. New Brunswick: Rutgers University Press. 1972. Pp. x, 335. \$12.50.

Dudley Carleton was one of the second-level political figures of James I's reign who was eventually to reach the top—a peerage and a secretaryship of state—by assiduous courtship of Buckingham. He is primarily remembered, however, as the holder of ambassadorships to Venice and The Hague, as the most prominent witness of Buckingham's assassination, and as the chief recipient of John Chamberlain's sharp and elegant letters. Maurice Lee has now given us a closer glimpse of this other half of the Chamberlain correspondence.

It should be said at once that Chamberlain's title as the best letter writer of his age is not endangered. Carleton's prose is often difficult going, and there are many tedious stretches. Lee was wise to omit the letters that do not discuss public events, though it is regrettable that he chose James I's death as his terminal date, rather than the end of the correspondence, which cannot have continued long, since Carleton returned to England in 1626 and Chamberlain died in 1627. But that is a small cavil with a book that is intelligently edited, admirably introduced, and carefully printed (with a mere two typographical errors).

That Carleton was a good ambassador cannot be doubted. Although some of his assessments were mistaken, more often he was right, as in his 1619 prediction (rather early among contemporary observers) that "this business of Bohemia is like to put all Christendom in combustion" (p. 270). His comparison of a dying Antwerp with a rising Amsterdam (pp. 212–13, 218) is a splendidly evocative piece of prose. And he had his moments of humor, most notably in describing an escaped bear's attempt to climb into his room in Venice (p. 158). But the bulk of the letters is devoted either to petty gossip or to diplomatic maneuverings. These are useful as a record for the scholar, but they possess little intrinsic interest. We gain hardly any sense of Carleton's personality beyond his snobbery, his ambition, his love of lavish entertainments, his scorn for magical dabbings, and his despair over his physical ailments.

Lee has helped all students of Jacobean politics by making this source readily available, thus rounding out what we already know from Chamberlain's letters, from Birch's *Court of James I*, and from Winwood's *Memorials*. Perhaps it is now time for a comprehensive history of James's diplomatic service, a group whose talents bear comparison with the skills of their famous Spanish and Venetian counterparts.

Nobody is better fitted for the task than Professor Lee, whose article on the subject (*AHR*, 72 [1966-67]: 1264-82) opened the way to a full-scale treatment and whose many related studies, not least this painstaking edition, render him ideally suited to replace Gardiner's timeworn analysis of James I's foreign affairs.

THEODORE K. RABB  
Princeton University

G. M. D. HOWAT. *Stuart and Cromwellian Foreign Policy*. (Modern British Foreign Policy.) New York: St. Martin's Press. 1974. Pp. viii, 191. \$10.95.

This book purports to treat English foreign policy from 1603 to 1688—a period of international and civil war in the British Isles and in continental states, increasing commercial rivalry between England and the United Netherlands, and (at its close) the accelerated aggressions of Louis XIV of France. Students could certainly use a good book on the subject.

But this is not that book. Howat's research seems limited to secondary works and a very few printed sources, to works and sources only in English, and to an indiscriminating selection of those. (He has apparently missed, for examples, Florence Evan's *Principal Secretary of State* [1923] and Pieter Geyl's *Orange and Stuart* [1939].) Though he prints the "secret treaty of Dover" in an appendix, he does not seem to have consulted the texts of other treaties in such collections as Jean Dumont's *Corps universel diplomatique*. These and other limitations hold the work down to an almost inconceivable superficiality and throw it into some substantial errors. We are told that the revolt at Prague in 1618 was a "national slavonic reaction" against the Habsburgs. We might be misled into believing that Charles II was always frank with the French ambassador about his intentions. Howat includes the Dutch republic among the members of the League of Augsburg in 1686; he also ignores Charles II's sending of English troops to Flanders as pressure on France to conclude the Peace of Nijmegen in 1678. Indeed, we are told little about James II's foreign policy as it applied to actual problems like the election at Cologne, the Anglo-Dutch dispute about Bantam, or the approach toward war by Denmark and Sweden about Hamburg and Holstein.

This book, in short, is an instance to prove that diplomatic history cannot be soundly written without close acquaintance with orig-

inal documents, often still in the archives, and the best secondary literature on the field.

GEORGE HILTON JONES  
Eastern Illinois University

HENRY ROSEVEARE. *The Treasury, 1660-1870: The Foundations of Control*. (Historical Problems: Studies and Documents, 22.) New York: Barnes and Noble. 1973. Pp. 219. \$10.50.

To the most casual London tourist, the vast unshakable edifice in Great George Street that is today the Treasury seemingly satisfies every definition of power and authority in administrative government. The social reality generated a mythology that is, in its way, almost as powerful. By the mid-nineteenth century, it had become to one tremulous civil servant, "the Dread-Department . . . which sat in judgement on them all and gave or withheld the means of encouragement to all" (Charles J. Cornish, *Sir William Flower* [1904], pp. 212-13). It suffered accordingly. To later generations it became less a bulwark of probity than a ministry of inertia, knowing the price of everything and the value of nothing.

In exploring this mythology, Dr. Roseveare's work has done much good. In this edited collection of documents he gives a sympathetic and sure-handed account of the creation of the administrative, political, constitutional, and "professional" base upon which financial control has been built. He succeeds, as Professor Elton promises in the series' introduction, in writing history as well as in helping the learner to know how history is written. His introduction closely describes the emergence of the Lords Commissioners, first as the administrative expedient of an imperiled Restoration monarchy, beleaguered by the debts and disasters of costly Dutch wars; then as an office of state, consolidating principles of formal accountability through successive attempts at parliamentary action; and finally as an organization of professional civil servants, crystallizing its authority through reorganization and reform in civil service procedure. One hundred pages of supporting documents, many drawn from nearly inaccessible sources, provide extended footnotes to the evolution and codification of the principles of Treasury control.

Roseveare ends his discussion with the beginning of the 1870s, a turning point in the Treasury's history, which finds a reformed and reconstituted office of state embarking upon a new phase of development. From this vantage point emerges a Treasury "streamlined and



economical . . . internally harmonious . . . and externally self-confident"; a lion rampant, larger than life. Yet, Roseveare does not neglect the lion's weaknesses; indeed, the problematic and tentative development of Treasury methods quickly disarms the most critical reader. Roseveare contrives to make *The Treasury* appear not so much the history of great doctrines and fixed principles, but a study in small innovations by dedicated men of skill and influence, capable of endless approximations and compromises, searching for ways to render a public accounting of public money.

On balance Roseveare's introduction admittedly leaves little time for the details and consequences of Treasury decisions, its patronage and parliamentary management, its fiscal policies, and its role in high finance. Similarly there is not much space for the struggles of "Thrift vs. Waste," or for studies of the Treasury in mortal combat against the civil servants and ministers of the great spending departments. This is perhaps unfortunate, as the Treasury seen in isolation is obviously not seen in perspective. Ultimately the greatest threat to Treasury control came not from weaknesses within, but with pressures from without, pressures derived from the imperatives of increasing legislation and expanding domestic, imperial, and naval policies. For the most part, however, these circumstances are well brought out in Roseveare's suggestions for further reading. Indeed, as a collection of documents, this book forms a convenient companion to the author's major study, *The Treasury: The Evolution of a British Institution* (1969), and should be read in conjunction with it. It may be argued that such a recommendation to buy two books, on similar themes, by the same author would bring a skeptical response from a good Treasury clerk. One hopes university librarians will see sound sense and good reason, and quietly "sanction without delay."

ROY MACLEOD  
*University of Sussex*

JOHN CARSWELL. *From Revolution to Revolution: England 1688-1776*. (Development of English Society.) New York: Charles Scribner's Sons. 1973. Pp. xxvi, 204. \$8.95.

No one looking for a brief account of England during the century before 1776 can do better than to buy and read Mr. Carswell's delightful volume. The style is lucid, witty, and elegant, the matter varied and succinctly described. To a ready acquaintance with the varied litera-

ture of the period and with modern works about it, he adds a perceptive sense of the past, a gift for vivid narrative and incisive phrase. A chronological table prefaces the thirteen chapters, and these are followed by statistical material, which could in view of its limitations have been omitted, and some suggestions for further reading. Almost half the pages are devoted to postrevolutionary England and the Augustan age; another five chapters concern mid-century, the "Robinocracy," and the long ministry of Sir Robert Walpole lying "like a huge flat stone at the bottom of English self sufficiency" (p. 2). The nature of party, of church and state, of class and economic interest is discussed. England was not deferential and, in spite of the stability achieved, was often turbulent in town, at football games, and in the remoter countryside. The better-known agitations that developed under George III are dealt with in the last three chapters. Discontent was not at first apparent; Englishmen, Carswell remarks, always viewed their constitution as perfect, whether explained by a Blackstone then or a Dicey later, just as major changes were going to be demanded or effected (143n). Failure of many to realize the transformations taking place already in so many ways was partly due to the gradual development of the "black" industrial society at the expense of the "white" agrarian way of life. The slow rate of depreciation must also be taken into account (pp. 75, 176, 180).

Carswell is readily quotable: the city-state of London; the populous, rural nation well defined except at the north by the sea (p. 13); his definition of the Commons as representing acres rather than bank balances (p. 41); and his commentary on the Whig slogan "liberty and property" as signifying the independence and power provided by landed estates rather than any more popular meaning (p. 15). He is excellent on domestic servants (pp. 18-19), on parish parson and squire (p. 136). He notes that the phrase "limited monarchy" was used by the Scots in 1689 (p. 6), and that the word "constitution" became current with the Revolution (p. 30). Franklin failed to remark the services rendered to the freedom of the press by the ugly, shameless Wilkes (pp. 4, 140).

There is room for criticism in the summary treatment of modern demographic and statistical analysis and in some claims, such as that for the innovative character of road making in the eighteenth century, but shortcomings of this sort and differences about judgment of character are more than compensated for by

the pleasure of reading thoughtful and well-written history.

CAROLINE ROBBINS  
Rosemont, Pennsylvania

ROBERT HALSBAND. *Lord Hervey: Eighteenth-Century Courtier*. New York: Oxford University Press. 1974. Pp. xiv, 380. \$12.50.

Sydney Smith's quip that it spoils a review to read the book can on occasion be reversed: it spoils some books to review them. *Lord Hervey* may be a case in point. The reviewer's pleasure is dimmed by the prescribed need to find something to criticize. To do so in the present instance, a few redundancies and trailing paragraphs apart, is virtually impossible. Professor Halsband has here climaxed an extraordinary preparation, as witness his perceptive, scholarly *Life of Lady Mary Wortley Montagu* (1956) and his definitive edition of her *Complete Letters*. Lady Mary's life, public and private, constantly intersected with Hervey's, and enhancing the significance of that intersection is the similarity of their minds and their reactions to events and individuals. They even excited similar reactions in others, notably Alexander Pope.

Never did a man live so long in a poisonous miasma as Pope's Lord Fanny, "that mere white curd of Ass's milk . . . painted Child of Dirt that stinks and stings . . . half froth, half venom . . . one vile Antithesis," a caricature inadvertently made more plausible by Lady Mary's division of the world into "men, women and Herveys," which in turn won a snicker when Horace Walpole applied it to the sexes. The caricature would scarcely have survived the life of the victim had not Pope's own sting and venom seized upon unmistakable features of Hervey's character and career. Indeed at times it seems as though a biography could take the form of explicating Pope's text, which is by no means to suggest that that text is true, particularly in regard to Hervey's public service.

What was that service? He entered Parliament in 1725 at the age of twenty-eight as burgess for the family borough, attaching himself, despite family pressure and Opposition enticements, to Robert Walpole. This attachment, immediately demonstrated by pamphlets, speeches, and votes, was promptly rewarded by appointment as vice-chamberlain of the king's household and membership in the Privy Council. Before long he became one of the most effective of Whig pamphleteers, pitching his "observations" in a calm, reasonable, and informed tone. This activity along with adminis-

trative efficiency gained him elevation to the Lords in 1733. Seven years later he became lord privy seal and as such joined the Cabinet Council. During the king's absences in Hanover he served as one of the lords justices who made up the regency. The fall of Walpole in 1742 meant also the fall of Hervey though not the end of his parliamentary activity, which continued until his death the following year. Simple in outline his career was nevertheless a tortuous, even tortured, one. When to the hazards of his public life, including his relationship to the discordant royal family (like Walpole he got on well with Queen Caroline), are added the vagaries of his private life—ill health, bisexuality, a ridiculous dedicatory epistle by Conyers Middleton, to name but a few—it is clear that his survival is itself a high tribute to his ability. That Halsband has knit all the strands of this complicated personality into a model biography is no less a tribute to his own ability.

CHARLES F. MULLETT  
University of Missouri—  
Columbia

ROSS J. S. HOFFMAN. *The Marquis: A Study of Lord Rockingham, 1730–1782*. New York: Fordham University Press. 1973. Pp. xiii, 397. \$15.00.

One of this country's foremost authorities on Edmund Burke now writes about the political career of his patron, the marquis of Rockingham, First Lord of the Treasury in 1765–66 and again, even more briefly, at the end of the American war in 1782. Swimming against a mighty stream of "Namierite" scholarship—including Paul Langford's recent and important book, *The First Rockingham Administration* (1973), which Professor Hoffman apparently had not time to read before concluding his own work—he seeks to re-establish the Old Whigs in their traditional role as guardians of the constitution and defenders at large of political virtue during the early decades of George III's reign. Full use has been made of the Wentworth-Woodhouse manuscripts and of the standard published sources, but the treatment of secondary material is perfunctory. The title is rather misleading, since the intention is to produce not a biography but a "piece of political history" (p. xii). The result is a useful study and a companion volume to, say, D. A. Winstanley's *Lord Chatham and the Whig Opposition* (Cambridge, 1912).

At risk of seeming to criticize Hoffman for not writing the book he specifically disavows, it must be stated as a matter of regret that the

author did not bring his great learning and knowledge of the sources more sharply to bear upon Rockingham the man. Overshadowed in his own lifetime and subsequently by more commanding personalities, Charles James Fox notable among them, the marquis surely lacked charisma. Adjudged timid, mild, and lazy by many contemporaries and later by historians, this sickly, short-lived Yorkshire grandee left his mark on the political history of his day and, for better or worse, determined the fate of his party. Hoffman is at his best when he adumbrates these facts; and in a perceptive passage he shows us Rockingham the magnate, expecting and exercising political power as a duty and right. Born into the plentitude of the Whig inheritance, he sacrificed his "intense relish for private life" to plunge into the struggle of contending factions. Decent, honorable, and well intentioned, the marquis had "neither ambition nor appetite for employing power" (p. 179), and Hoffman candidly admits and describes the flaw and its sad meaning for the Old Whig party.

There was a more serious deficiency, however; Rockingham lacked both the will and the intellect to formulate high policy or to oversee its execution. It is scarcely a matter of surprise that the party he led became the *enfant perdu* of British politics. Its successes—for example, the repeal of the Stamp Act—were fragile and transient, not because of the machinations of a Bute or a Mansfield, but in no small measure because Rockingham and many of his friends found opposition more congenial than taking up the burdens of government. Burke's myth of secret and malign influences merely embellishes an explanation already full and sufficient.

CHARLES R. RITCHESON  
University of Southern California

WALTER J. SHELTON. *English Hunger and Industrial Disorders: A Study of Social Conflict during the First Decade of George III's Reign*. [Toronto:] University of Toronto Press. 1973. Pp. ix, 226. \$15.00.

This meaty book is a valuable contribution to the social history of England under George III. Professor Shelton examines the causes and character of the riots that disturbed the English provinces in 1766 and then spread to the metropolis in 1768 and discusses the reactions of the authorities. Basically, two adverse sets of economic conditions converged after 1763: a general postwar depression, exacerbated by colonial commercial protest against the Stamp

Act, causing unemployment, underemployment, and falling levels of wages; and a series of inclement seasons that created a scarcity of grain and meat. Other major contributory causes were postwar demobilization; a population growth that at this point overshot the country's capacity for food production; and the enclosure movement. This last everywhere deprived more of the poor of their ultimate reserve resource, their rights of common, and in the East Midlands, owing to the spread of sheep grazing, it drove much labor off the land altogether. Insofar as destitute provincials sought jobs in the metropolis and added to the pool of underemployed, the London outbreaks were to some extent a delayed sequel to those in the provinces. The author draws together useful information about the Thames coal-heavers, illustrating their relationships with employers and their attempt to establish a "closed shop" in 1768, and he presents evidence that ex-army veterans and Irish Whiteboys played a significant part in the rioting, especially in London. Most interesting is his treatment of problems arising from the growing sophistication of the English economy. Middlemen were becoming essential; yet the traditional prejudices against them remained strong (as they still do among the ranks of English labor); and the instinct of both the populace and the authorities was to apply outmoded remedies—price and wage fixing, the enforcement of open selling in local markets, and strict application of old laws against the middleman's operations of forestalling and regrating. This traditional reaction made matters worse: it checked the movement of food supplies from the farms; and the assumption that the mobs were right to enforce sale at "fair prices" caused magistrates to delay measures for the enforcement of law and order. Shelton perhaps goes beyond his evidence when he asserts that the gentry consciously turned the laborers against the middlemen and wealthy farmers in order to escape having all these groups line up against them as they had done during the militia riots of 1757.

IAN R. CHRISTIE  
University College London

THE MARQUESS OF ANGLESEY. *A History of the British Cavalry, 1816 to 1919*. Volume 1, 1816 to 1850. Hamden, Conn.: Shoe String Press. 1973. Pp. 336. \$15.00.

The first marquis of Anglesey was in command of the cavalry at Waterloo where he lost

a leg. The current marquess, seventh of the line, having written a biography of his ancestor, has now turned to a "definitive history" of the cavalry from 1815 to 1919 in four volumes. This first installment, covering the years down to 1850, without important engagements to detail, is able to devote about half its length usefully to the social life of the cavalry, its training and equipment, and its employment in support of the civil power in England. Most of the balance is a "trumpet and drum" account of the actions of the cavalry in India, against Nepalese, Marathas, Afghans, and Sikhs. The virtues of this old-fashioned work are the ones it ascribes to Fortescue—that it digests a great deal of material carefully and tells its story readably, if episodically. These are not inconsiderable virtues.

The defects of the latter half are also familiar from Fortescue. At the heart of this work is a sentimental affection for the final century of the horse cavalry regiments and a lack of interest in the political context in which they operated. As a result, though this work will please antiquarian students of the British army, it is not likely to satisfy professional historians. War is a whole. The study of its parts must be directed by a firm analytical conception or a controlling question, beyond what will suffice for a regimental chronicle. To the military historian the study of a separate service rarely makes sense unless the period under consideration is one of change in technology or technique. Anglesey acknowledges this difficulty but, in this first volume at least, with a relatively brief time span to consider, he does little to overcome it. Resistance to innovation before the Crimea and the Mutiny is noted, but barely explored or explained. Perspective is absent on other counts. Anglesey tends to take over uncritically the viewpoint of the Victorian officers. Thus he is able to write of the second Maratha War as one of them might have done at the time, "Throughout the war the enemy had proved more or less contemptible in battle but always cunning in evasion" (p. 208). Professional judgment on this work should, of course, remain tentative until its design is completed in the final volume. In the meantime, at least everyone will enjoy the handsome illustrations.

S. J. STEARNS  
*Richmond College,  
 City University of New York*

JOHN BROOKE and MARY SORENSSEN, editors. *The Prime Ministers' Papers: W. E. Gladstone*. Vol-

ume 2, *Autobiographical Memoranda 1832-1845*. (Royal Commission on Historical Manuscripts.) London: H. M. Stationery Office; distrib. by Pendragon House, Palo Alto, Calif. 1972. Pp. xvi, 293. \$16.00.

This is the second of four projected volumes of selections from the Gladstone Papers to be printed by the Historical Manuscripts Commission. The first volume consisted for the most part of retrospective autobiographical memorandums written in old age. It made interesting reading but was of limited value to the historian because the events described were based on the memory of a very old man. Volume 2, now under review, covering thirteen years of Gladstone's public life, is composed mostly of contemporary memorandums written very shortly after the events they describe, evidently for personal reasons. Consequently it is far more valuable than the first volume. Subsequent volumes should be even more significant, since they will contain selections from the papers of a more mature statesman, closer to the center of political affairs.

John Morley made good use of the documents printed in this volume, although he often took liberties with his quotations. It was impossible, however, even in a biography as long as Morley's three fat volumes, to quote more than a small fraction of what is reproduced here. Moreover, Morley often tells his story from other sources, such as Gladstone's letters to his father and his wife, which often closely parallel the accounts in these memorandums.

The documents in this volume—up to the year 1838—are mainly of biographical interest, many of them being summaries of conversations that Gladstone had with various well-known literary and political figures of the day. In 1838 he appears to have been brought into what was in effect Peel's "shadow cabinet," and from this point on until his resignation in 1845 the memorandums deal mainly with the political issues of the day with which Gladstone was involved either as a confidant of Peel's or as a minister, and after 1843 as a cabinet member. It is not surprising that Gladstone was somewhat chagrined at not being invited to join the cabinet in 1841, since he participated in two secret meetings with Peel and other party leaders in the preceding week. He concludes philosophically, however, that at the Board of Trade he may anticipate "plenty of homely useful work." Gladstone was slow to recognize, as the editors point out, the full significance of his appointment. They continue: "Peel did well to steer him away from the



politics of ecclesiasticism. . . . As he admits himself there was a touch of fanaticism about his religion at this period which confirms the wisdom of his father and Peel in steering him on to secular matters. What Gladstone wanted was a hard spell at practical business and a rest from theorizing. He buckled down to his work, and although he never mentions it he soon became the effective head of the department." Gladstone won the respect of his colleagues for his obvious ability and integrity, but as the editors point out there was still an element of "naivety and gaucherie" that must have tried Peel's patience at times. Few young ministers can have threatened to resign as often as he did on matters of principle, and, of course, within two years of entering the cabinet he did resign over a scruple that few could understand. The volume contains a wealth of detail on the issue of the enlarged Maynooth grant and Gladstone's difficulties with it.

The introduction summarizes the contents of the volume with some brief but penetrating comments, as indicated in the quotation above, but there is no attempt at general analysis. The editing is likewise limited, with little if anything in the way of explanatory footnotes (in contrast to the excessive annotation in the first two volumes of *The Gladstone Diaries*). We are left to guess what motion, bill, or speech the document in question is referring to. This suggests that these volumes are being published for the student who is supposed to know, rather than for the general reader. Undoubtedly it will be useful to the specialist to have these key memorandums from the Gladstone Papers in print, but he will still have to go to the bulk of unpublished papers in this mighty collection to supplement them. It is unlikely that the 750 folio volumes in the British Museum collection will ever be published *in toto*, but even were this done there are still, we are told, another fifty thousand documents in the Gladstone Papers at Hawarden. Obviously these rich collections will continue to be mined by historians for many years to come.

J. B. CONACHER  
University of Toronto

J. T. WARD. *Chartism*. (Studies in Economic and Social History.) New York: Barnes and Noble, 1973. Pp. 286. \$12.00.

The author of the first general history of the Chartist movement since 1920 modestly declares in the introduction that "it would be arrogant to claim to supplant the narratives

of Hovell and West." Why? In general the book is an updated and improved version of Mark Hovell's *The Chartist Movement*, which was first published in 1918. J. T. Ward's *Chartism* is also a catalog of mostly ephemeral organizations, a chronicle of mostly minor gatherings and mostly unimportant events. Lists of leaders appear here and there in Old Testament style. Political history predominates, and the reader may wish for more in the way of social history, analyses, conclusions, and insights that utilize some of the more fashionable historical tools from sociology and psychology. He might also wish for a shorter introductory section. One chapter is entitled "The Antecedents," another "The Background," another "The Foundations," and, at last, "Emergence." They total well over a third of the book and contain too much ordinary textbook history.

Dr. Ward also shares Mark Hovell's strong appreciation of Lovett's "noble" and "intellectual" Chartism and joins in loathing "cowardly" physical-force Chartists. Feargus O'Connor receives another good drubbing and emerges as a "convincing liar" suffering from megalomania. Only in dealing with the Land Plan does Ward express sympathy for O'Connor's efforts. Perhaps some day an Irish historian may come along who can skillfully interpret O'Connor's blarney and brilliance, which was used so effectively to play between physical and moral force in order to harness both impulses. Among other Chartists held up for criticism, Bronterre O'Brien is declared to be "machiavellian" at one point in his career. His prison sentence "cleared his head," according to Ward. George Julian Harney, the friend of Marx and Engels, is said to have suffered from a "personal schizophrenia." Other Chartists are criticized for not following their democratic precepts, but in general the lasting importance of the movement for British democracy is given short shrift. The revolutionary potential of Chartism is minimized, although the author admits that the sources never will allow certainty in assessing the Chartists' violent plans.

The great advantage of Ward's *Chartism* over Hovell and West is that he is able to draw upon almost fifty years of additional piecemeal research on the movement. He does so quite well, pointing out such things as the role of Dr. Black, the nature of the alliance with unionism, and conflicts with the Anti-Corn Law League; and he does not lose sight of the local diversity of Chartism throughout his narrative. The resurgence of 1847 and 1848 and

the aftermath of Chartism, as well as the fate of individual Chartists, are well handled, which cannot be said of the earlier histories. Short and apt quotations from a variety of Chartist sources enliven an otherwise clear but undramatic narrative. While Mark Hovell's liberal chronicle has been superseded by this up-to-date book with a similar style and a similar point of view, many may think that G. D. H. Cole's *Chartist Portraits*, a string of well-written biographies, may still serve as the best introduction to Chartism.

HENRY WEISSER  
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UTZ HALTERN. *Die Londoner Weltausstellung von 1851: Ein Beitrag zur Geschichte der bürgerlich-industriellen Gesellschaft im 19. Jahrhundert.* (Neue Münstersche Beiträge zur Geschichtsforschung, number 13.) Münster: Verlag Aschendorff. 1971. Pp. vii, 397. DM 64.

This study, which comes from the doctoral program and research series of the University of Münster, provides an in-depth account of the origins, operation, and significance of the Great Exhibition.

How it inspired a continuing movement, stimulated an interest in and new educational programs for science and technology, caused nations to seek solutions to postal, patent, and other international problems, and created a curious relationship between science and art are discussed in some detail. But Haltern is primarily interested in deepening past treatments of this subject, and for him the exhibition stands symbolically at the outset of the second half of the nineteenth century as an exordium to later trends and developments. The gathering was triumphantly middle class, at once a means of promoting its own self-consciousness and providing an outline for the new world industrial society it was creating. Together with its many successors, the exhibition offered tangible evidence of the idea of progress and suggested the possibility of "one world"—a perfected and united world civilization.

Haltern raises again the hopes then centered in the peace movement, in the belief in the unifying qualities of the Christian tradition and free trade, and in the vision of a mechanized society, most of whose members would be freed to devote themselves to the "higher purposes" of human existence. Unfortunately the exhibition promoted national as well as international self-consciousness, imperialism

and competition as well as cooperation, and the utopian industrial society failed to emerge in the form anticipated by the nineteenth-century dreamers.

The dash of Hegel here and Marx there, the hint of Spengler in its outreach, as well as a profusion of sociopsychological terms do not contribute to ease of translation in some places, but Haltern's style is enthusiastic, even eager, and it encourages one to persevere while he searches for complex, underlying social and cultural historical interrelationships.

Certainly anyone who reads this work must thereafter consider the Great Exhibition as more than a mere landmark in Britain's economic history and a monument to an admirable prince who was undervalued in his own time.

WILBUR DEVEREUX JONES  
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DONALD N. MCCLOSKEY. *Economic Maturity and Entrepreneurial Decline: British Iron and Steel, 1870-1913.* (Harvard Economic Studies, volume 142.) Cambridge, Mass.: Harvard University Press. 1973. Pp. xiv, 157. \$11.00.

Dr. McCloskey's study of the British iron and steel industry in what have been characterized as years of relative decline (1870-1913) is a valuable contribution to the historiography of the period. His study is firmly grounded in the contemporary trade and technical literature, but at the same time it brings modern methods of analysis to bear on the available data, with illuminating results. There are three stages to McCloskey's argument. First, he identifies and analyzes the hypothesis of entrepreneurial failure and two related explanations for relative decline: the "burden of the past" argument and the argument from the effects of slow growth of demand. Having defined the origins and logic of the "entrepreneurial failure" argument—itsself a valuable exercise in historical criticism—the author goes on to examine the British iron and steel industry during this period, interpreting the data to establish the competitive structure of the industry, the composition of demand, the industry's response to new technological opportunities, and its productivity record. A whole series of assumptions about the industry are shown to have been too readily accepted: McCloskey's findings are that the structure of the industry in Britain did not buttress poor entrepreneurship and that in situations which have been uncritically accepted as touchstones of entrepreneurial failure—the delay in adopting steel in shipbuilding, in the



adoption of the basic process, or in the exploitation of basic ores—British entrepreneurs followed an inherently rational course. Finally, having established the productivity record of the British industry, McCloskey compares this with American experience and concludes that when the comparison is made on an equivalent basis, American and British productivity records were indistinguishable.

This study contributes to the literature in a number of ways. First, as a technological and economic appreciation of the British iron and steel industry it supplements and corrects the existing industrial histories and by its methodology helps to make parts of the industry's history more explicit. Second, it is a substantial contribution to the wider economic history of Britain during this period, not only as part of a reappraisal of overall economic performance, but also as a cogent demonstration of the importance of regional factors in the British economy. Third, as an example of historical method, McCloskey's book has wide relevance: in a negative sense, by showing how easily the qualified conjecture of the monograph can be translated into the bald assertion of the textbook, this will be a salutary commentary for teachers; but in the more positive sense, this combination of firmly based historical research and modern techniques of analysis, coupled with a clarity and economy of style, is itself a useful example. Finally, McCloskey's findings and approach raise questions about the nature of interpretation in economic history which go beyond his cautious comment that "the natural desire to affix blame . . . is liable to become an obstacle to understanding if it is combined with inadequate measures of performance." McCloskey may have exonerated the traditional scapegoats, but before the textbooks take refuge in the bland concept of "inevitability," those who interpret British economic history would do well to take stock and define their terms. As an exercise in specification, this study is an instructive example: substitutes for the hypothesis of entrepreneurial failure, while they may be more robust, will not have the same appealing simplicity.

M. C. REED  
University of Glasgow

W. ROSS JOHNSTON, *Sovereignty and Protection: A Study of British Jurisdictional Imperialism in the Late Nineteenth Century*. (Duke University Commonwealth-Studies Center, number 41.) Durham, N.C.: Duke University Press, for the Center. 1973. Pp. vii, 357. \$9.75.

When Queen Victoria died, she had been empress of India for over a quarter century, and in that same period "Her Majesty's Dominions" had multiplied threefold. But it was still unclear in law whether "Dominions" included Britain's African and Pacific protectorates. Protectorates were fairly new dependencies, and their creation was part of the growth of jurisdictional imperialism of the late nineteenth century. The empire made by British lawyers is the subject of W. Ross Johnston's book. He has traced the development of jurisdictional imperialism through the legal opinions of the Colonial Office, the Foreign Office, the Law Officers of the Crown, and the Lord Chancellors. As British jurists came to grips with the requirements of law and order on the frontiers of their world, they had to revise their legal theories of sovereignty and protection.

Traditional extraterritorial jurisdiction as exercised in the Ottoman Empire and China was not easily applied to Africa and the Pacific because in the eyes of Europeans it was not possible to find many "civilized" governments with whom to sign the necessary treaties giving extraterritorial rights to consular officials. As Englishmen in greater numbers went to Africa and the Pacific, friction between them and the "natives" increased so that the problem of law and order was perhaps the first to be raised in official circles. Some sort of protection was necessary not only for British subjects but even more for those Africans and Pacific islanders who were being exploited and enslaved. In the absence of British extraterritorial jurisdiction, British offenders might escape all punishment.

The solution to this problem was eventually the spread of British jurisdiction, but the process was a halting one made up of *ad hoc* measures by which legal theory and practice were revised to enable extraterritorial jurisdiction to be extended to what were regarded as "uncivilized" areas of the world. By the 1880s Britain exercised jurisdiction over her subjects who were within reach of British consuls, and their reach was a long one. By virtue of the implicit consent of the signatories to the agreements drawn up at the Berlin Conference, the British government claimed jurisdiction over foreign nationals in British-protected areas. And in the 1890s legal and eventually administrative jurisdiction was asserted over Africans and Pacific islanders. Jurisdictional imperialism was a fact.

In keeping with the nature of British expansion, the spread of jurisdictional imperialism was in part the result of reluctance to assume financial responsibility. Therefore the simple legal

expedient of annexation—the direct assertion of sovereignty—was for the most part rejected, and the protectorate was devised as a cheap, limited dependency. British jurists, unlike German and French, maintained throughout most of the century the distinction between sovereignty and protection so that British officials in theory were to exercise minimal legal responsibility. But in practice by 1900 they were interfering regularly in the internal affairs of the people in the protectorates above and beyond the demands of law and order, so that the jurisdictional empire was becoming part of the formal empire.

Johnston handles this complicated subject with facility and clarity. But he seems to have adopted as his own the nineteenth-century terms of reference to “the civilized British and the savage primitive peoples [usually his term is “natives”] in Africa and the Pacific.” Linguistic habits die hard, and so do the ideas they reflect. Johnston’s apparent insensitivity to the meaning of these words, his easy use of them without quotation marks or a trace of irony, cannot help but call into question his historical assumptions and damage what should be an important addition to British imperial and legal history.

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SYDNEY H. ZEBEL. *Balfour: A Political Biography*. (Conference on British Studies Biographical Series.) New York: Cambridge University Press. 1973. Pp. viii, 312. \$14.95.

Arthur James Balfour was wealthy, elegant, clever, and well connected. His uncle, Lord Salisbury, was prime minister of Great Britain for longer than Gladstone. In the Conservative party Balfour trod a primrose path for fifty years, succeeding his uncle as prime minister, and, after an unhappy spell as party leader, luxuriating on to the 1920s as the very archetype of the elder statesman. There have been earlier biographies. The present volume is modest alike in aspiration and achievement. The author rests for justification on the fact that “no professional historian has hitherto attempted a balanced account or appraisal” of Balfour’s political career. What follows is a dutiful rehearsal of the several phases of that career, sometimes using the appropriate manuscript sources, but more often relying upon published authorities. There is no pretense of going beyond existing studies in dealing with

particular episodes and problems; instead, the interpretation offered here is always derivative. The book will chiefly be useful to the general reader with a pronounced taste for the inner life of politics, or to students approaching the subject for the first time. Scholars interested in the period will find nothing new here. And they will regret the book’s failure to penetrate below the surface of events: so that—to take three examples—there is no proper examination of how Balfour emerged as Salisbury’s successor, of his treatment of the tariff reform issue as party leader, or of the extent of his responsibility for the assault on the Dardanelles in 1915. The even flow of the narrative is never disturbed. Although Professor Zebel shows no undue partiality toward Balfour, his book matches its subject in its urbane manner and propensity to evade difficulties.

P. F. CLARKE  
University College London

STEPHEN ROSKILL. *Hankey: Man of Secrets*. Volume 1, 1877–1918; volume 2, 1919–1931; volume 3, 1931–1963. London: Collins; distrib. by St. Martin’s Press, New York. 1970; 1972; 1974. Pp. 672; 608; 688. \$12.50; \$15.00; \$15.95.

C. J. BARTLETT. *The Long Retreat: A Short History of British Defence Policy, 1945–70*. [New York:] St. Martin’s Press. 1972. Pp. xiii, 306. \$14.95.

The three-volume biography by a retired naval officer turned historian is Victorian in scope, length, and attention to detail. The essay in contemporary history by an academic, while informative, bears vestiges of the tutorial-with-sherry style. Taken together, the two works afford an opportunity to grasp the dimensions of British national security policies from the turn of the century to the present day.

The subject of Captain Roskill’s much acclaimed biography served at the very center of the national security policy-making machinery for most of the period between his appointment as an assistant secretary of the Committee of Imperial Defence in 1908 to his dismissal as a minister by Churchill in 1942. His concurrently held offices included secretary to the cabinet, to the Committee of Imperial Defence, and to the chiefs of staff subcommittee thereof. He was also chief secretary to the British delegation for most of the major international conferences from the first 1915 precursor of the Allied Supreme War Council to the Imperial Conference of 1937. That Hankey could so long maintain his position under a variety of taskmasters

invites explanation. The thread of the story is told mainly by Hankey himself in the form of diary notes and letters to his wife.

The major theme of volume 1, British preparations for and conduct of the 1914-18 war, has been treated in recent years by a number of historians, as well as by Hankey himself in his full dress yet muffled *Supreme Command 1914-1918* (1961). The overall picture is not substantially changed by the present publication, but there are a number of interesting tidbits together with a sense of immediacy that make the book well worth reading even to the nonspecialist. The evident breakdown of the war machinery in mid-1915 pushed Hankey into what might have been a policy-making role: "I find that the P.M. talks as though I was in actual charge of the Dardanelles operation, which is very embarrassing, as I have no real responsibility whatsoever." The reorganization of December 1915 effectively put war control in the hands of the major operating commands, whence Lloyd George found it difficult to wrench power back. Hankey remained the impenetrably loyal man of secrets. While expressing dismay at the Somme offensive in private, he approved it in a general review in his official capacity as secretary of the War Committee. In similar style eighteen months later, in the famous Maurice debate, Hankey noted that Lloyd George had withheld the fact that he had been apprised by the War Office of corrected figures on manpower strength on the Western Front. "This knowledge embarrassed me a little when M.P.s of all complexions kept coming up to the official gallery to ask me what was the 'real truth.' There was a general recognition of the fact that L.L.G.'s speech was magnificently got up and that this was due to me."

Volume 2 begins with the virtual apotheosis, for both Britain and Hankey, resulting from the turnabout victory of 1918. In his view London was now "the real directing centre of world politics," and as Hankey elsewhere testified, the "British Empire interested me more than the peace settlement." As is well known, the empire's interests were faithfully served at Paris, and Hankey declined the secretary generalship of the new League of Nations. The United States had, however, emerged as "the most powerful nation in the world." The British therefore accepted with grace American proposals on naval disarmament and the extinguishment of the Anglo-Japanese Alliance. More pigheadedly, Lloyd George, with Hankey as loyal agent, went on to disaster over the

inability to enforce the Turkish settlement. "A nice position," Hankey commented after Lloyd George's fall, "when the British Empire cannot stand up to Turkey!"

At the end of 1922, Hankey was confronted with the first major challenge to his position since the creation of the cabinet secretariat six years earlier. In the struggle with the Treasury for the future of the central administrative services, Hankey emerged largely triumphant, while the "national security" bias of that machinery was thereby confirmed. Over the fiasco of the occupation of the Ruhr, his advice to the new prime minister, unlike that of the cabinet, was "simply to watch events." With the reparations conferences of 1924, which embodied the policy of Anglo-French-German reconciliation, Britain returned to the shaping of history. Further British commitments to the Continent evoked unsuccessful resistance. "I am unhappy about this Four Power [later Locarno] Pact. . . . If I were behind Lloyd George to brief him what hay we would make of it!" He had been more successful in quashing the proposal for a Channel tunnel. Will this ever be built?

The remaining years of the decade show Hankey somewhat on the defensive: against his subordinate and rival Tom Jones on the secretariat, against naval disarmament, and against the recurrent proposals for a unified Ministry of Defense. The hard-working Hankey, ensconced in his privileged niche, was becoming, according to Roskill, somewhat dehumanized. Yet I can testify to Lord Hankey's courtesy in extreme old age to an unknown American graduate student.

The latest and largest volume can hardly be summarized within the limits of the present general review. Its greatest interest to students will be the mass of information on the rearmament effort of the thirties. Hankey here threads his way amidst the influence of disarmament and economy, interservice rivalries, imperial isolation and appeasement, and the specter of an air-dominated war against three major antagonists. His retirement in 1938 at the age of sixty appears to have been partially a capitulation to the proponents of a stronger central defense organization.

Recalled at the outbreak of war to government service as minister without portfolio, Hankey noted the existence of a "present passive conflict with Russia" even before the latter's attack on Finland. The relationship with Churchill was unhappy, and Hankey was less vital than in younger days. His dismissal accom-

panied and preceded criticism of Churchill's leadership on both personal and strategic grounds. In retrospect, Hankey's emphasis on a maritime strategy at the expense of the strategic bombardment effort appears well founded.

Bartlett's is a well-written study, based of course largely on the public record, including a few but informative memoir sources. It is particularly to be commended for its attempt at presenting the whole picture, including such often-neglected topics as equipment and recruiting. The interesting features include the positive pursuance of the American connection, the enforced concentration of military forces in Europe, and what might be called the symbolic value attached to what gradually became transformed from imperial defense to world role. The economic constraints and penalties become ever harsher. Once again, the impression is of prolonged rear-guard action, but this time toward—what? Military nuclear capability was developed in response to a rationale both military and political: the latter the "Great Power" syndrome. Yet by the late 1960s the only real utility was to hold open the possibility of future development into a viable force. "The actual degree of credibility and strength possessed by the current British nuclear force therefore mattered less than its existence." *A policy of illusion?*

One of the major aspects of British policy emphasized by this work is the continued importance attached to the Middle East, on the control of which in 1947 the chiefs of staff insisted to the point of threatened resignation. Adherence to this view contributed to the 1956 Suez fiasco, comparable to that of 1922 at Chanak. In contrast to these failures, the collapse of the British-sponsored Federation of South Arabia and the evaluation of Aden in 1967 have a certain far-off, comic opera quality. The consequence of continued clinging to the shadows of the past may indeed be tragedy repeated as farce.

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ALBERT MARRIN. *The Last Crusade: The Church of England in the First World War*. Durham, N.C.: Duke University Press. 1974. Pp. xi, 303. \$9.75.

Albert Marrin has collected a splendid series of quotations from sermons, periodicals famous or obscure, newspapers, and books. He has well used the papers of Archbishop Davidson at

Lambeth. We hear the prelates and the pastors of England reacting, partly as Englishmen and partly as Christians, to the German onslaught of 1914, the invasion of Belgium, the news of atrocities in France and Belgium, the idea of the war as a crusade, the diagnosis of German Protestantism as responsible for errors in German attitudes to the state, and the desire for war aims that should include the making of a more just society and the achievement of lasting peace. To account for what happened he has summarized the development of the Church of England since 1870, especially in its relation to society. The material on the churches and the suffragette movement is especially interesting.

The book is stronger in collecting the quotations than in enabling the reader to understand the atmosphere of the age. The author perhaps assumed more knowledge of a context than some of his readers will possess; but whether this is so or not, the effect is sometimes to lift the examples out of the general area that would enable it to be seen in perspective. Moreover, he allows himself many adverse judgments against people or utterances. He does not agree with Mandell Creighton that moral judgments are out of place in historical writing; and since his attitude comes near to assuming that an authentic Christianity will be pacifist, he finds it difficult always to be patient, not merely with the absurd utterances of the bishop of London, but with almost anyone who urged men to fight on Christian grounds. This is not true of the final conclusion, where a perception of moral complexity and a generally balanced judgment go together. In that conclusion he can praise even a would-be tyrannicide like Dietrich Bonhoeffer, who was willing to go far further in "Christian violence" than the warriors of an army and their supporters. Every student of the churches in war, or of the consequent moral difficulty, will find matter here to interest him. There is a useful bibliography.

OWEN CHADWICK  
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Cambridge

MICHAEL KINNEAR. *The Fall of Lloyd George: The Political Crisis of 1922*. [Toronto:] University of Toronto Press. 1973. Pp. xiii, 317. \$15.00.

It is a truism, though a valuable one, that contemporaries are usually not aware of the broad trends that later generations take for granted.

Michael Kinnear has set out to establish that in the eyes of British politicians in 1922, it was by no means clear that Lloyd George and the Liberal party were on the way out and that the days of coalition were over. If some of Kinnear's conclusions are less novel than he likes to think, he has nonetheless done a thoroughly professional job in which his studies of the actions and sayings of politicians at the center are integrated into an invaluable analysis of politics at constituency level. He argues that the Conservative victory in the 1922 election was unexpected, but that the famous Carlton Club decision whereby the Conservatives withdrew support from, and thus brought down, the Lloyd George coalition was less unpremeditated and less due to the intervention of such politicians as Stanley Baldwin than has often been maintained. Both Lloyd George's immense strengths and his failures to carry conviction are well explained: "Sometimes Lloyd George had a consistent policy, but an inconsistent explanation of it." Rightly, but with tedious repetition, Kinnear insists that it was Asquith, motivated by desire for personal vengeance, who effectively split the Liberal party. In places appalling printing errors have reduced the author's workmanlike prose to gibberish; worse still, a misplaced zeal for economy has restricted the insertion of references to the ends of paragraphs, often with confusing results. Possibly Kinnear overstates the case when he declares the election of 1922 "one of the most interesting, and certainly the most influential, of this century." He sees Lloyd George and the Liberals, henceforth lost to government office, almost as another lost generation. Still, this is an excellent book that, particularly because of its basis in electoral statistics, in many respects supersedes Lord Beaverbrook's more dramatic account; it also forms an admirable complement to Maurice Cowling's *Impact of Labour, Nineteen Twenty-Two-Twenty-Four: The Beginning of Modern British Politics* (1971). Yet, whether a new synthesis is really called for, I am less certain: the broad trends were there, even if contemporaries, as always, could not perceive them.

ARTHUR MARWICK  
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BRIAN BOND, editor. *Chief of Staff: The Diaries of Lieutenant-General Sir Henry Pownall*. Volume 1, 1933-1940; volume 2, 1940-1944. [Hamden, Conn.: Archon Books, 1973; 1974. Pp. xxxii, 399; xxiii, 216. \$16.50; \$14.50.

Henry Pownall was a highly competent staff officer who filled a series of increasingly responsible posts from 1933 until his retirement, due to ill health, in 1944. He joined the staff of the Committee of Imperial Defence in 1933, became director of military operations and intelligence at the War Office in 1938, and chief of staff to the British Expeditionary Force (B.E.F.) upon the outbreak of war. The publication of his diaries, well edited by Brian Bond, is an important event because Pownall was both a well-placed and a clear-sighted observer, who recorded events in an incisive and vigorous fashion. Unlike many published diaries, which merely provide color and atmosphere, Pownall's genuinely illuminate the course of events. The hesitancy that marked British rearmament is mercilessly chronicled, particularly the obstinacy of Ramsay MacDonald and the growing ineffectiveness of Maurice Hankey. The infighting at the War Office in 1938-39 and the reluctant acceptance by the Chamberlain cabinet of the necessity for a Continental commitment are described in detail. This is the most important part of the first volume, and Pownall's account will be essential to future historians, because it balances and corrects the plausible but very misleading version of these events found in the Liddell Hart memoirs. Pownall also does justice to Lord Gort, both as C.I.G.S. and later as commander in chief of the B.E.F. His diaries, and J. R. Colville's biography, *Man of Valour* (1972), should complete the rehabilitation of Gort's reputation from the slurs cast upon it by Liddell Hart, Alanbrooke, and Montgomery. Pownall's account of the withdrawal of the B.E.F. from the Dyle to Dunkirk makes absolutely clear that only Gort's determination and nerve saved the B.E.F. in the face of wildly unrealistic orders from both London and the demoralized and disintegrating French high command. It also makes plain that whatever Churchill may have said to the French about parity in evacuation, the B.E.F.'s chief of staff was determined to save as much of the British army as he could. Unlike Gort, Pownall's military career did not end with Dunkirk. After a variety of assignments in England, he was posted to the Far East, and his assignments there provide the interest of the second volume. He was chief of staff to Wavell's ill-fated American-British-Dutch-Australian (ABDA) command. When it was all over he trenchantly summed up the causes of the British debacle: "We were gambling on their not coming into the war—yet." After further interludes in Ceylon and Persia,



Pownall's last assignment was as chief of staff to Mountbatten's Southeast Asia Command (SEAC). Put there by Sir Alan Brooke, the C.I.G.S., to keep Mountbatten "on the rails," Pownall found his chief a trial, further compounded by Wingate, Stilwell, and the Chinese. The diary gives a matchless picture of the cross-purposes and near chaos at the top that bedeviled the Allied effort in SEAC.

Unfortunately little of Pownall's personality comes through, although Bond tries to remedy this in his introductions. The impression left is of a shrewd, choleric, and often very intolerant man who, however, did historians a considerable service by recording those crucial events in which he took part.

RAYMOND CALLAHAN  
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R. W. THOMPSON. *Generalissimo Churchill*. New York: Charles Scribner's Sons. 1973. Pp. 252. \$8.95.

R. W. Thompson, the author of several popular accounts of the Second World War, has written a new study of Winston Churchill as a warlord. Covering only the years 1940-42, which Thompson says encompassed the brief period of Churchill's dominance over both the Americans and his own generals, this analysis represents an exceedingly personal history.

Thompson's pre-eminent hero is clearly General Sir Claude Auchinleck, whom Churchill hectored and abused so mercilessly during the Western Desert campaign in 1941-42 before dismissing him ignominiously on the eve of final victory at El Alamein. Auchinleck's replacement, General Sir Bernard Law Montgomery, is foremost among Thompson's many aversions. Monty is described as a hypocritical braggart of excessive caution in combat. Thompson admits, however, that Montgomery performed the role expected of him by the British government and public.

Rather more originally, Thompson is bitter against Churchill on the grounds that the prime minister vastly underestimated British power and resources, giving in too much to the Russians and Americans. Whether the courageous prime minister actually had much real choice on this issue remains a moot point, to say the least. On the question of what Thompson, in agreement with so many others, deems Churchill's "evil genius" in the heavy bombing program, namely Professor Frederick Lindemann, Thompson quotes General Paget: "He [Churchill] preferred to seek and take

advice from people like Cherwell [Lindemann], Harris, Wingate . . . than the Chiefs of Staff." It is unquestionably true that his fellow-adventurers, rebels and romantics, appealed to Churchill far more than his too frequently modest and reticent advisers, both in and out of uniform.

If Thompson has proved his case that the Churchill of the First World War fiascos remained the man of those in Norway, Greece, and Africa in 1940-42, his sources are decidedly limited and almost entirely obvious. Moreover, the vital War Cabinet minutes and memoranda for this period, at last released from the Public Record Office, are never cited. Well-known photographs and maps of irrelevant date are also included in this essentially superficial work.

TRUMBULL HIGGINS  
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MICHAEL FOOT. *Aneurin Bevan: A Biography*. Volume 2, 1945-1960. New York: Atheneum. 1974. Pp. 692. \$13.95.

Many biographies put the case for the defense; Foot puts the case for the prosecution. Bevan's great rhetorical strength lay in attacking the forces of conservatism with the deftly handled weapons of logical argument, and, while Foot shows how misleading it is to call him a demagogue, he also takes up the attack where Bevan left off. The great battles for socialism against Morrison in the late forties and against NATO and the Gaitskellites in the early fifties are described with relish, and the struggle over the Campaign for Nuclear Disarmament (which Foot helped to found) is told with a melancholy that leaves it uncertain how much of the loss of zest that appears in the chapters about Bevan's last years was due to his failing health and how much to his biographer's distress at having to disagree with the man he had loved and followed for so long. Foot sees his hero as a giant nibbled to death by committees, though he does not realize that it is necessary to explain the committees in order to make the giant comprehensible. The chapters about setting up the National Health Service show that Foot can explain political and administrative problems without compromising his pro-Bevan position, and it would be useful if he had brought the same talent to the chapters about rearmament. Bevan's true worth cannot be assessed properly on the basis of a statement of one side of the complicated argument about British capacity, American policy, and Russian



intentions—Bevan's contribution was more sophisticated than his twice-quoted remark that the Russians would not attack because they manufactured only thirty million tons of steel a year, and the cold warriors did have some arguments on their side.

Foot is not as happy defending Bevan as attacking his enemies, and not as skillful at defense as Bevan himself would have been. To explain the failure to resist the 1947 housing cuts by saying that his resignation might have brought down the government (when it had a majority of almost two hundred) is not a dextrous way to defend a man who later resigned when the government had a majority of only six. Bevan's remark that the Tories were lower than vermin is defended as a casual phrase, but Laski's guess that the phrase cost Labour two million votes is then treated as a serious psephological estimate; Foot seems not to realize how well Laski reflected the alarm a good many socialists felt at Bevan's capacity to rouse the Conservatives for battle.

In a way Foot has returned to an old-fashioned style of political biography in which the virtues of the hero are dwelt upon and his faults are minimized or ignored. Blake's *Disraeli*, with its tribute to Gladstone in the closing pages, is perhaps too austere a model, but, while Foot's two volumes are good enough to mean that nobody will do the job again soon, they are not so good that there is no room for another even better life of Bevan.

TREVOR LLOYD  
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HAROLD MACMILLAN. *At the End of the Day, 1961-1963*. New York: Harper and Row, 1973. Pp. vi, 572. \$16.50.

This is the sixth and final volume of Harold Macmillan's memoirs. For those who know the earlier volumes there will be few, if any, surprises. The tone, as always, is measured; there are appropriate eulogies of particular colleagues, and every effort is made to minimize difficulties within the Conservative party or within the Atlantic alliance. Still the story of the years 1961-63 cannot be told without some reference to certain of the strains that became increasingly conspicuous during the period and that attracted very considerable attention in the press. Thus, for example, Macmillan is obliged to explain why he felt compelled to ask for Selwyn Lloyd's resignation as chancellor of the Exchequer in July 1962, and why he thought it well to conceal his purposes in a more gen-

eral overhaul of his government. Maintaining a certain reticence even a decade after the events he describes, Macmillan alludes very generally to the "lack of initiative" shown by the chancellor and to his "fatigue" after long service in two very arduous posts. The sentiment, as always, is generous; the author is clearly concerned to avoid giving offense. Still it is impossible for him not to acknowledge that the larger reorganization of the government, which earned him the unflattering "Mac the Knife" reputation, was simply an elaborate concealment of his dismissal of the chancellor. With the advantage of hindsight, Macmillan implies that he might have done better to ask only for Selwyn Lloyd's resignation, sparing the others, thereby wounding a single individual but avoiding a political uproar.

There are no comparable "second thoughts" about his relations with Charles de Gaulle. Because so many accounts have circulated about Macmillan's meeting with de Gaulle at Rambouillet in December 1962, and its possible influence in causing the French president a month later to veto the United Kingdom's petition to enter the Common Market, the encounter has historic importance as great perhaps as the meeting that followed between Macmillan and President Kennedy at Nassau. Macmillan's account of both meetings suggests that he feels no remorse about any of the positions that he took. From the evidence that he presents, it is obvious that de Gaulle had reached his decision about British entry into the Common Market before Macmillan ever set foot in France. Nothing that happened at Rambouillet or at Nassau, where the Skybolt missile was formally buried and the Americans offered Polaris in its place, persuaded de Gaulle to shut the door to Britain. That decision was already implicit in the way the French president—always courteous and correct, but not very giving—treated Macmillan in their Rambouillet meeting. The exclusion greatly saddened Macmillan, but he was not disposed to be bitter; the day for British entry was simply postponed. He implied that no good purpose would be served by acrimony. This, in a sense, is the message of all these volumes, which are, at one and the same time, civilized, gracious, and bland.

STEPHEN R. GRAUBARD  
*Brown University*

ELISABETH ISRAELS PERRY. *From Theology to History: French Religious Controversy and the Revocation of the Edict of Nantes*. (International Archives of the History of Ideas, 67.)

The Hague: Martinus Nijhoff. 1973. Pp. x, 254. 55 gls.

As a case study in the polemical uses of history and the way men search the past to find weapons for present conflicts, this book should interest many scholars who are not concerned either with seventeenth-century France or with Reformation historiography as such. The author's lucid exposition of the context of the historical controversy shows the interplay of current events with the themes and variations of the many polemicists on both sides, and her biographical and bibliographical appendixes make available in organized and useful form much information previously scattered and never analyzed as a body of material. The leading controversialists are tested by the criteria of history (set forth in ch. 3) with the result that the patterns emerge clearly.

Among Perry's contributions to Reformation historiography, most significant is probably her documentation of the reasons for the superiority of Protestant writers, despite their methodological problems (p. 130) and their ambivalence about their sixteenth-century predecessors (e.g., p. 47 and ch. 6 *passim*), which continues to handicap twentieth-century French Reformed scholars. Because of their willingness to recognize historical change, the Protestant writers were able to write better secular history than the Catholics. "What the Catholics could only deplore, the Reformed were willing to explain and analyze" (p. 142), and because they were on the defensive they had to be scrupulous in the use of sources, thus advancing the critical sense necessary for the historian's discipline (e.g., pp. 178-80).

The link between the most able Protestant polemicists, especially Bayle, and the philosophes is spelled out more fully and more reflectively than in previous studies (ch. 7). The same can be said of the contrasts between Bayle and Jurieu (pp. 101-03, 193-98), which reflect the dilemmas and elucidate the various choices of seventeenth-century French Protestants.

Students of the France of Louis XIV will be interested in the key position of Bossuet. Perry's analysis of the failure of the Reformed to convince their Catholic contemporaries that they could reconcile their faith with loyalty to the French monarchy, and her evidence that both parties were unhappy with the trends of their own time, especially the increasing domination of the state. Basically, the attempt to "justify" (or condemn) the past on historical grounds failed. In later generations religious polemics in France would return to theological

grounds and the civil wars would become the concern of secular historians whose bias, if any, would be nonconfessional.

NANCY L. ROELKER  
Boston University

P. JANSEN. *Arnauld d'Andilly, défenseur de Port-Royal (1654-1659): Sa correspondance inédite avec la Cour conservée dans les Archives du Ministère des Affaires Étrangères.* (Bibliothèque de la Société d'Histoire Ecclésiastique de la France.) Paris: Librairie Philosophique J. Vrin. 1973. Pp. 133.

Paule Jansen has already contributed significantly to the history of French Jansenism with *Le Cardinal Mazarin et le mouvement janséniste français, 1653-1659* (1967). It was the chief merit of that volume to undertake a convincing explanation of Mazarin's determination in attacking the Jansenists during the 1650s. Most authorities, both old-fashioned and modern, have vaguely implied some fear on Mazarin's part of the political disloyalty of the friends of Port-Royal, but little convincing evidence of collusion between Jansenists and *frondeurs* has ever been produced. Jansen has demonstrated that Mazarin's policy was wholly political and cynical: that he manipulated the Jansenist issue to curry favor with the papacy; that he sought to buy papal support cheaply without having to yield on any of the diplomatic or political issues that he really held dear. This thesis is solid, and while some critics have deplored Jansen's emotional defense of the Jansenists and her moralistic condemnation of the French court, the author's passion in no way has detracted from her contribution.

It is with a little less confidence that one may praise Jansen's current volume, a collection of letters between Robert Arnauld d'Andilly and the French court during the years 1654-59. From his retreat at Port-Royal des Champs, Arnauld d'Andilly, the brother of Dr. Antoine Arnauld, undertook a defense of the Jansenists in the years immediately following Innocent X's Bull *Cum Occasione*. During these years Mazarin had the condemnation of the Five Propositions from the *Augustinus* received in France and encouraged the pope in a final condemnation of the whole of the *Augustinus*; it was also the years that saw the censure of Antoine Arnauld by the Sorbonne and the appearance of Pascal's *Lettres provinciales*. During all of this, Arnauld d'Andilly used his good offices at court to plead for a sympathetic understanding of the Jansenist position under the bitter attacks of the Jesuits and

their friends. Jansen shows that Mazarin cleverly exploited Arnauld d'Andilly's concern for his fellow *solitaires*, for his brother Antoine, and for the future career of his son Simon (later to become marquis de Pomponne) to gather materials with which to inflame the religious dispute—all to serve his political purposes. Even Antoine Arnauld's provocative *Lettre . . . à une personne de condition . . .* and *Seconde lettre . . . à un duc et pair de France . . .* were indirectly provoked by Mazarin. Throughout his correspondence with the court, Arnauld d'Andilly never seemed to lose his confidence in Mazarin because in his earnest, Christian patience he never could suspect the Machiavellian intentions of the cardinal-minister.

Jansen thus supplements her earlier book, but she does not add a great deal of historical value. What she adds may upset her critics, for she amply provides more ammunition for an emotional defense of the purity of the friends of Port-Royal and a bitter condemnation of Mazarin and other practitioners of *raison d'état*. Still, there is no little poignancy in an intimate viewing of the uneven match between the old-fashioned Arnauld d'Andilly and the worldly Mazarin. Perhaps what is deficient here in historical interest is compensated for by the material's human interest.

NORMAN RAVITCH  
University of California,  
Riverside

AGNES ETHEL MACKAY. *La Fontaine and His Friends: A Biography*. New York: George Braziller. 1973. Pp. vii, 9-227. \$8.95.

Biography as a genre has been for some time out of favor within the community of professional historians, however great its enduring popularity among the general reading public. Significant studies such as Wilson's *Diderot* remind us how much can be accomplished within the form by a scholar of intellectual rigor and historical sensitivity, but, on the whole, the trend toward prosopography reflects the deeper theoretical and substantive interests of current historians. Agnes Ethel Mackay's latest biography will do nothing to lessen such tendencies.

*La Fontaine and His Friends* neither uses its main figure as a foil to the intellectual and cultural life of seventeenth-century France nor (its stated goal) makes us "alive to the climate and tendencies of the time," so that we might disengage what is unique and personal to the life, thought, and accomplishments of the poet.

Impressionistic, anecdotal, and ever credulous, the book is gossip at third hand. All the faults of the genre at its weakest are manifest. Events, feelings, and motives impossible to know or verify are presented as gospel. Character traits are seen as essential, constant, and forever reflected in extant paintings. Anachronism is unavoidable because mental states are assumed to be precursory in men of genius. Social analysis is superficial and fifty years out of date.

Almost all of the historically interesting problems upon which a rigorous life of La Fontaine might shed some light are ignored or treated cursorily and out of proper context: the nature of patronage and protection; the process of change in literary taste; the coexistence of piety and libertinage; the role of the Académie Française; the quandary of the Protestant nobility under Louis XIV; the perception of old age and illness in the early modern era. The book's most ambitious exercise, a chapter on the aged La Fontaine's conversion, is commonplace, a paraphrase of La Fontaine's hagiographers, effectively unrelated to the patterns of belief of the broader milieu. A brief but painful exercise in pure intellectual history, an attempt to demonstrate how the *Fables* support Gassendist versus Cartesian views of animal life, serves ultimately to launch a plea against cruelty to animals and, a fantastical undertaking, to link La Fontaine's views to those of the twentieth-century scientists Jean Rostand and Charles Noel-Martin.

In short, *La Fontaine and His Friends* will be of almost no utility to the historian or student of seventeenth-century culture. It provides neither analysis nor insight, and we truly do deserve at least one or the other from the writers of biography.

ALAN CHARLES KORS  
University of Pennsylvania

FRANÇOIS CARON. *Histoire de l'exploitation d'un grand réseau: La Compagnie du Chemin de Fer du Nord, 1846-1937*. (École Pratique des Hautes Études—Sorbonne. VI<sup>e</sup> Section: Sciences économiques et sociales, Centre de Recherches historiques. Industrie et artisanat, 7.) Paris: Mouton. 1973. Pp. 619. 78 fr.

Business history has not been highly developed in France, largely because of the extreme secrecy with which the French guard their business activities. In recent years this secrecy has abated somewhat, in part, because more and more businessmen have come to believe that the attitude of the public toward them would be more favorable if based on hard

evidence rather than upon imaginary data and, in part, because with the nationalization of the railways, the large commercial banks, and at least one large manufacturing establishment (Renault), scholars can claim access to records that are, in essence, public property. Economic historians like François Crouzet, Maurice Lévy-Leboyer, and Bertrand Gillet have moved into this interesting field and have also steered many students to it. The work before us is a product of their endeavor.

The dissertation on the history of the Nord Railway by François Caron is not easy reading, but it is a mine of information and a rich collection of insights. Fortunately the author provides a "general conclusion" of sixteen pages, which is a thumbnail sketch of his entire story. At the outset of this synthesis Caron states his central theme: "To write the history of a business is to write the history of costs. There is no true entrepreneur except the one who is able to modify them. The entire history of the Nord Railway is a progressive loss of control by the management of its costs." Some of this loss is attributed to the necessity of borrowing large amounts of capital for construction or for the introduction of technical improvements; some arises from the fact that railways provide a service that, within broad limits, has to be maintained in good times and bad (it cannot be stored); some from arrangements with the state that infringed upon the company's freedom of action; some from the policies of trade unions that kept labor costs high and inflexible (they were some sixty-five per cent of the costs of running the road); and some from the development of a rigid bureaucracy within management. When French railways became nothing more than a public utility, nationalization was a logical outcome.

This story makes the book worthy of study, but one will find tucked in here and there a wealth of reflections about the role of railways in determining the location of industry; how small lines were a drag on the main lines; and how technical innovations created problems of financing and profit making. In brief, this book is an absolute must for the student of railway history and a quasi-must for students of French economic history.

SHEPARD B. CLOUGH  
Columbia University

MARCEL GILLET. *Les charbonnages du nord de la France au XIX<sup>e</sup> siècle*. (École Pratique des Hautes Études—Sorbonne. VI<sup>e</sup> Section: Sci-

ences économiques et sociales. Centre de Recherches Historiques. Industrie et artisanat, 8.) Paris: Mouton. 1973. Pp. 508. 84 fr.

The cover photograph on Marcel Gillet's book depicts a bronze sculpture of coal miners. It is a misleading choice of artwork, however, for the text within scarcely discusses the people who worked the mines. Although Gillet interviewed (in the late 1950s) some 117 men who had worked in the mines of Nord and Pas-de-Calais before 1914, his principal source has been the records of private companies. No less than eighteen mining firms gave him access to their papers, and consequently his study deals largely with questions of production, investment and reinvestment, business structure, and the organization of the *patronat*. Gillet's story is one of increasing concentration and—toward the end—integration with the larger metallurgical companies. In general, traditionalistic business practices predominated, and the principal mine owners were more concerned with growth than with development—with maximizing profit by tried-and-true methods rather than experimenting with organizational innovations. They came to the idea of regional cartelization rather late—1901—and while they recognized that cooperation might serve them well in the growing conflict with organized labor, individual firms still remained jealous of their independence. Throughout, they were committed first of all to maintaining high prices rather than to increasing production, though by the early twentieth century the area's mines were also producing two-thirds of all France's coal.

It is perhaps appropriate to the subject that such generalizations must be extracted from the book by some strenuous digging. This study's primary value, and apparently Gillet's primary interest, resides in the mountains of data he has provided: there are twenty-five pages of excellent maps, forty-five pages of graphs, and forty-five different tables. It is very much history without any of the human juices in it and written largely—though not uncritically—from the company point of view. But for all of that, it is still an extremely useful source for specialists.

GEORGE FASEL  
University of Missouri—  
Columbia

GUY LAPERRIÈRE. *La "Séparation" à Lyon (1904–1908): Étude d'opinion publique*. Preface by ANDRÉ LATREILLE. (Collection du Centre d'Histoire du Catholicisme, number 9.) Sherbrooke:

Université de Sherbrooke; Lyon: Centre d'Histoire du Catholicisme. 1973. Pp. 220. \$10.00.

Guy Laperrière's analysis of the different manifestations of Catholic and anticlerical opinion in Lyon illuminates both the immediate reaction in France to the separation of Church and state and its long-term impact. The separation led to numerous violent confrontations around France in 1906 between Catholics and the state agents who applied the legislation. Lyon was, at first glance, a likely spot for violence. The religious cleavage was fundamental; with both an archbishop and a growing working class, Lyon was a center of Catholic spiritual resurgence, but at the same time it elected anticlerical politicians. Moreover, this cleavage appeared to be growing wider, as indicated by a brawl in 1903 in which one Catholic was killed. Yet the city remained peaceful in 1906. Laperrière finds the explanation in the deliberate moderation of the state agents, the archbishop, the clergy, and much of the press.

The separation had little apparent impact on the people of Lyon. It was a consequence of the triumph of anticlerical opinion, but in Lyon it did not itself lead to a further decline in Catholicism. The number of nonreligious burials had been increasing since the turn of the century and would stabilize with separation. The number of youths choosing priesthood fell dramatically after the separation, but it had already been declining for several years and would begin to increase in 1911. Moreover, the pattern of voting remained the same from 1900 to 1912, suggesting that the divisions among the people of Lyon, which were expressed temporarily in religious terms, had a deeper social basis. The First World War, Laperrière argues, intervened to prevent any more precise appreciation of long-term effects on public opinion.

Laperrière's book is an outgrowth of the *thèse de troisième cycle* that he wrote, under the direction of André Latreille, at Lyon's Centre d'Histoire du Catholicisme. Its main interest is not his conclusions, which confirm the usual interpretation of the separation, but his methods. He shows how a wide variety of sources—including daily newspapers, election returns, administrative reports, public statements of officials, and records of personal decisions (such as marriages and ordinations)—can be utilized to study public opinion. He did not, however, analyze this opinion in terms of social, economic, and geographical factors or compare Lyon with other areas, in particular

with an area in which violence occurred. It is hoped that he will pursue the investigation that he has so skillfully begun with this unpretentious monograph.

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WILLIAM A. HOISINGTON, JR. *Taxpayer Revolt in France: The National Taxpayers' Federation, 1928-1939*. (Hoover Institution Manuscripts in Microfilm Series 3.) Stanford: Hoover Institution Press, Stanford University. 1973. Pp. xxi, 212. 1 reel microfilm \$15.00.

This book is a workmanlike description of the origins, acts, and decline of the *Fédération Nationale des Contribuables*, the leading French taxpayer protest group of the 1930s. Hoisington traces the organization of the *Fédération*, analyzes its programs, describes its attempts to influence government and municipal policies, and then compares it with the postwar *Poujadist* movement. He seems to have used all the proper sources: interviews, personal papers, and public documents. The data he has collected are valuable.

Unfortunately the author makes no serious attempt to judge the wisdom of the economic and political program of the *Fédération*. He suggests that nobody in French politics knew much about economics in the 1930s, and I would agree; but his implication (p. x) that Keynesian theory might have furnished solutions to French problems simply shows that economic illusions are quite as persistent as they ever were. Fiddling with unbalanced budgets would have done no good at all.

The real lack in this book, however, is feeling, understanding, empathy. The organization Hoisington describes was made up of thousands of frightened people. They were not tools of big business, nor were they themselves particularly evil, and dismissing their rather incoherent cries of pain as "fascism" tells us nothing. They were victims as pathetic as the workers whose organizations they so abhorred. But their humanity gets short shrift in this book. This particular argument over tax policy was the stuff of tragedy. The author must have sensed something of this, but he has rigorously excluded it from his book, perhaps on account of some misguided idea of objectivity. I hope that in his next work he will include at least a little of the human dimension.

CARTER JEFFERSON

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Boston



BRIAN CROZIER. *De Gaulle*. New York: Charles Scribner's Sons 1973. Pp. ix, 726. \$12.50.

The publishers call this "the first complete biography." It may well be the last of the old biographies. Complete in the sense that it covers the life 1890-1970, it is external and circumstantial in a way that a second generation of biographies may not be. A lengthy chronicle, recalling public events especially of the final thirty years, it is less than a close examination of the man. Risking condescension, one might say that it is a skillful compilation from excellent books, well written if not always well proportioned, about as full a life and times as most readers will wish. The attitude expressed is mixed: "The personality, the will and the skill, the erudition and the self-discipline, the memory and the courage: these things still kindle the admiration. But the harm he did the West, and therefore France, outlives him."

The discussion reveals shakinesses. It is not easy to see how the events of February 6, 1934, could be interpreted as "a mini-Commune." To have the Allied withdrawal from Norway bring down Daladier's government on March 21, 1940, is to confuse both French and European affairs. The description of the Franco-British entry into Belgium that year makes no sense. Elevating de Gaulle's prime foe, Maxime Weygand, to the dignity of marshal betrays a capacity for missing nuances. There is no evidence that Georges Mandel was tortured by the Gestapo. On the other hand, judgment of the Front Populaire as "an almost unmitigated disaster" is at least arguable, and belief in the "largely spontaneous" nature of Algiers' Moslem population's demonstration for France on May 19, 1958, is conceivable. But only a biographer of that other rebel might care to write: "Though less ruthless than, say, Franco toward his enemies, in the mass . . . [de Gaulle] was less forgiving towards followers who had turned against him."

Many matters are well treated: for instance, de Gaulle's contribution to military thought, the bitter Syrian affair, the difficulties with Admiral Muselier, and the opening up of the Indo-China war. A few pieces of fresh information emerge from private papers seen or conversations held. But for the most part Crozier has not pushed his researches beyond secondary materials and the general's writings and speeches. For the early life he follows the French biographers; for the Second World War he is content to cite de Gaulle's memoirs much of the way, with additions from Churchill, Eisenhower, Robert Aron, and some others. For

the more inaccessible period of the second coming and the "elective monarchy" he depends again on the biographers and on Carmoy, Vianson-Ponté, Newhouse, Alexandre, and other French and English writers. His work must be judged as synthesis and summation. Nowhere does he offer the kind of illuminating discussion of the Fifth Republic that we have from such analysts as Philip Williams and Martin Harrison. Crozier is first and foremost a reporter. He has experienced some things and read about more. There are limits to what a busy and productive journalist can consult. He might, however, have improved his inadequate account of the 1946-58 exile by consulting the diaries of Claude Mauriac and C. L. Sulzberger.

The book is a generous and readable record, even if "history" should not retain the tentative and puzzling conclusion that "as a writer, [de Gaulle] wrote too little; as a soldier, he fought too little; and as a statesman, he came too late."

JOHN C. CAIRNS

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R. E. M. IRVING. *Christian Democracy in France*. London: George Allen and Unwin. 1973. Pp. 308. £5.50.

The French literary critic Albert Thibaudet once compared the Radical-Socialist party, the backbone of the Third Republic of his day, to a radish—red outside, but white inside. Its counterpart in the history of the Fourth Republic was the Christian Democratic Mouvement Républicain Populaire (MRP). Condemned by its critics as "poissons rouges dans un bénitier" (or as "a party with a red head and a white tail"), the Christian Democrats bear a singular resemblance to the Radicals in that, like their predecessors, they too were unable to reconcile doctrinal purity with the realities of political power; they too failed under the Fourth Republic's last prime minister, the Catholic Pierre Pflimlin, to save the regime from a right-wing take-over; and their political fortunes, again not unlike that of the Radicals', serve to illustrate the dilemmas that confront any center party in French parliamentary life. But here the resemblance ends, for the MRP represents the first successful attempt since the Revolution of 1789 to create an authentic fusion between Catholics and Republicans by means of a broad-based political formation drawing its support, initially at least, from those Catholic elements in French society that had hitherto remained resolutely opposed to any kind of

compromise with *la Gueuse* and the democratic process.

In this carefully researched and neatly presented monograph, R. E. M. Irving traces its origins to the early nineteenth-century efforts of Lamennais, Lacordaire, and others to reverse the traditional hostility of the Catholic Church to the democratic legacy of the Revolution, explaining how the innate conservatism of the French Catholic electorate also contributed to undermining any attempt to launch a progressive Catholic party that showed any signs of trying to come to terms with the modern world. With the establishment of the Third Republic, which was itself based on a militant anticlericalism, such efforts as *le Ralliement* followed by Marc Sangnier's *Le Sillon* (1899–1910) proved to be even more difficult to carry out into practice, and although the Christian Democratic parliamentary group founded in 1924 could muster at its peak some nineteen deputies in the National Assembly, it was not until the Liberation, when all the conservative parties lay under the shadow of collaboration, that Christian Democracy, now organized as the MRP, finally came into its own as the only Catholic party that could claim an outstanding resistance record.

The bulk of Irving's book presents a detailed account of the meager accomplishments of the MRP from its spectacular electoral triumphs in 1945 and 1946, when it succeeded in polling almost five million votes corresponding to twenty-six per cent of all the ballots cast, through the setback it suffered in the 1951 election, when most of its conservative and Catholic clientele defected to the new Gaullist party (RPF), to its pathetic demise as a political party in 1963, without neglecting by way of conclusion a very thorough and illuminating discussion of its rather futile reincarnation during the Fifth Republic in the form of Jean Lecanuet's Centre Démocrate. The author devotes a chapter each to Christian Democratic policies in social and economic matters (noting that its only notable achievement here lay in the area of improved housing conditions), agricultural reform, foreign affairs (where Robert Schuman's supranationalist ideals in the early fifties gave way to a more moderate position in support of a policy of European integration by gradual stages in the sixties), and the most divisive issue of all, decolonization, which paved the way for the collapse of the Fourth Republic and de Gaulle's return to power. Like the regime with whose destiny the MRP's fortunes were so inextricably intertwined, the

record of the party was ultimately a record of almost constant failure to adjust to changing conditions, both at home and abroad. For despite its avowed purpose immediately after the Liberation of carrying out a vast program of social and economic reforms ("la révolution par la loi"), the gulf separating the progressive outlook of some of its leaders from a backward, largely rural electorate prevented the party from achieving any fundamental structural transformations in a society that remained stubbornly wedded to the past.

ALAIN SILVERA

Bryn Mawr College

JOHN FRASER RAMSEY. *Spain: The Rise of the First World Power*. (Mediterranean Europe Series, 1.) University: University of Alabama Press, for the Office for International Studies and Programs. 1973. Pp. 341. \$10.50.

A compact new history of Spain from pre-Roman times to the Catholic kings would be a welcome companion volume to the two excellent works on the epoch immediately following, J. H. Elliott's *Imperial Spain* (1963) and John Lynch's *Spain under the Habsburgs* (1964). But Professor Ramsey's book, I regret to say, does not come up to the standard set by those studies. He gathers his information from other twentieth-century general Spanish histories by English, American, and Spanish writers (these are usually in English translation), with an occasional excursion to William Prescott. He does not re-examine any of the ancient sources of his topic; the few references to older chroniclers come via quotations from Roger B. Merriman or Prescott. One finds here viewpoints from and quotations of (in addition to the historians mentioned) Jaime Vicens Vives, Américo Castro, Henry Kamen, and many others, but the author contributes no original research. Yet, on the dust jacket he addresses himself (or, on his behalf, the publisher does) to the "sophisticated general reader and the specialist." It is unlikely that specialists would want to add this derivative book to the list of Spanish historical studies. And the general reader, sophisticated or not, would probably prefer the superior books, all easily available, of the writers quoted above to Ramsey's rather pedestrian narrative.

Still, even a nonoriginal book might have its educational uses as a synthesis, but Ramsey organizes the material too unevenly for such a purpose. He explains that his own point of view requires a disproportionate arrangement of the various historical periods: in order to

study the process of Spain's development from earlier anarchic conditions into a world power, he allots over half of the text to Isabella and Ferdinand. Apparently convinced that his special focus is original enough to warrant recounting the famous story, the author stresses the fact of how the royal couple emerged from the unique circumstances of the reconquest as the builders of the Spanish state and how the forces forged in the reconquest produced the phenomenon of the conquistadors. Further belaboring the all too obvious, he tells us that thus the basis was laid for Spain's greatness, but in his view the Habsburg dynasty—whose "foreignness" he emphasizes with particular regret—"did destroy the greatness of Spain." What can one say to such an oversimplification? Apart from the fact that today the "greatness" of a nation-state, past or present, is not necessarily being equated with its military and economic power, a multiplicity of reasons caused world domination to slip from the grasp of Spain. The parentage between the monarchs in Madrid and Vienna, if it was a reason, would have been the least of them. Long before the decline of Spain the Spanish branch of the house of Austria had lost any foreign aspect in the person of Philip II, that symbol of Spanishness. Contrary to Ramsey's claim, Philip never contemplated sacrificing Spanish interests in order to "support the particular plans and projects" of the imperial Austrian branch of the family. The correspondence between the Spanish and Austrian courts overwhelmingly shows the opposite to be the case.

ERIKA SPIVAKOVSKY  
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MANUEL FERNÁNDEZ ALVAREZ, edited with introduction and notes by. *Corpus Documental de Carlos V*. Volume 1 (1516-1539). Salamanca: Consejo Superior de Investigaciones Científicas, Universidad de Salamanca. 1973. Pp. 562.

It is somewhat surprising yet very encouraging to see a scholar in our day willing to devote the years of painstaking labor required to locate, collect, edit, and publish an extensive body of source documents. But those who are acquainted with the work of Manuel Fernández Alvarez, *Catedrático de Historia* at the University of Salamanca, know his dedication to meticulous and exacting scholarship. They also know the insightfulness of his interpretations of sixteenth-century Spanish history. He believes in imaginative and creative historiog-

raphy, but also in the necessity of basing that historiography on the careful study of primary sources.

This is a stout volume of 228 hitherto unedited documents of the emperor Charles V between 1516 and 1539, gathered mostly from the rich Archivo de Simancas. There have been previous collections of Charles V's letters published—mostly in the nineteenth century and mainly by non-Spanish scholars—but none has provided a more useful body of workable material for interpreting the life and policies of the man who played the key role in European affairs for forty years. Notable by its absence in the earlier collections of Karl Lanz, L. P. Gachard, Gotthilf Heine, and Johann von Döllinger is the correspondence of Charles V with members of his family. These form the nucleus of the present *Corpus Documental*.

The largest and most significant division of this work includes some ninety-eight letters to the Empress Isabel written by Charles V during his many peregrinations across Europe. These documents reveal a strong paternal concern for his far-flung kingdoms and a guarded yet unmistakable tenderness toward his family. They illumine the emperor's affairs in Germany, the Netherlands, Italy, and Franche-Comté, as well as Spain itself, and they underscore his preoccupation with the Turks. Events of particular prominence in the documents include the emperor's journey to Italy and Germany in 1529-30, following the Peace of Cambrai with France in 1529, in order to receive the papal coronation at Bologna and attend the Diet of Augsburg, where Charles hoped to end the Lutheran schism and unite the empire against the growing Turkish threat. The emperor's endeavors to establish and maintain order in Italy are also shown, as well as details of his administration of Castile and Aragon through the regency of his wife. Other documents are devoted to his crusade against the Turks, especially the attack on Tunis in 1535, to the affairs of Rome and Italy in 1536, and to the negotiations for an alliance with France in 1538.

There is much information here about the political structure as well as the operation of Charles V's government, and students of socioeconomic history will find useful economic data and insights into daily life. Above all, the *Corpus* demonstrates the crucial position of Spain in all European affairs of the time. Professor Fernández should be congratulated on his concise and appropriate annotations, and the Consejo Superior de Investigaciones Científicas, the Universidad de Salamanca, and the Funda-

ción Juan March should also be commended for sponsoring this valuable publication.

DE LAMAR JENSEN

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MIGUEL MARTÍNEZ CUADRADO, *La burguesía conservadora (1874-1931)*. (Historia de España Alfaguara, volume 6.) Madrid: Alianza Editorial Alfaguara, 1973. Pp. 592.

This ambitious work deals with much more than the evolution of the Spanish bourgeoisie following the collapse of the First Republic. It is an unusually comprehensive social history of the Restoration regime, a richly quantitative and methodologically sophisticated probing of the class and institutional foundations of the Canovite system. Major sections deal with the consolidation of the Restoration polity, demographic and economic trends, the system of social classes, and the role of political parties and labor organizations from 1890 to 1931. Though often critical of the dominant men, parties, and arrangements of Restoration Spain, the analysis is nevertheless remarkably measured and objective.

Integrating most of the relevant research produced in Spain and abroad during the last decade and a half, this is primarily a work of synthesis. It embodies no all-embracing historical interpretation but is permeated by a sociological outlook and by a conviction as to the primacy of economic forces. Professor Cuadrado exemplifies the preoccupation of many younger Spanish historians with relentlessly quantitative social and economic analyses, and he seems to share their relative disdain for traditional political and narrative history. His work subordinates the flow of historical narrative to a series of topical-analytical or topical-historical dissections. The result is a very complexly organized and penetrating study, but one nearly as lacking in a sense of movement as the semi-moribund regime it anatomizes.

There are few surprises in this slowly flowing, immensely detailed, and labyrinthine work, which in general sustains (as it clarifies) the accepted picture of the Restoration system. We are left in no doubt that that system was profoundly oligarchical, that land ownership was concentrated, that the tax system was regressive, that the dogmatic anti-interventionism of the liberal-bourgeois state slowed economic growth, and that the regime, through the *turno pacífico* of its two dynastic parties, failed to move toward a mobilization of the masses that would have secured its foundations. What is most

valuable, however, is the wealth of statistical data brought to bear on these (and many other) realities, permitting a quantitative appreciation of them not matched by other general histories of the Restoration era.

A recurring theme of *La burguesía conservadora* is the stability of the social structure in Spain from 1874 to 1931, a stability based on economic retardation and the continued supremacy of the *haute bourgeoisie*. The latter are condemned for their failure to carry out their "historical mission" to liquidate the *ancien régime* in Spain and for their propensity (even as they adhered to a doctrinaire liberalism) to seek accommodation with traditional elements in society, most particularly with the nobility whose survival and, indeed, resurgence during the Restoration years is convincingly analyzed.

The author's sympathy lies with the emerging working classes, and he provides an extensive and, on the whole, reliable survey of the labor movement from its origins in the nineteenth century to 1931. But as a social historian, Cuadrado is not greatly concerned with ideological questions, and he does not delineate as thoroughly as he might the various tendencies within Spanish labor or make clear the full impact on them of the world war and the Russian Revolution. Despite perfunctory references to the *Trienio Bolchevista*, he does not really evoke the exaltation and revolutionary overconfidence that affected large sectors of the Spanish working classes in the postwar period. And it is chiefly for this reason that his discussion of the bourgeois "reaction" of 1919-23 does not illuminate as it should the social polarization that resulted from working-class *élan* and middle-class anxiety—a dialectic that operated in Spain somewhat as it did in Italy and Germany. Thus the bourgeoisie did not turn toward repression and Primo de Rivera merely from a vague "fear of Soviet Russia" but in response to the whole prior quasi-revolutionary surge of a working class aroused by the war and inspired by the Bolshevik Revolution. In this regard, the author deals less than adequately with the origins and meaning of *pistolismo* in Barcelona in the postwar period, neglecting, for example, the intimations of labor leaders such as Pestaña and Buenacasa that the workers were, in fact, the first to resort to systematic terror and that the Confederación Nacional del Trabajo fell for a time under the domination of a terror machine whose objectives were only remotely related to the legitimate aspirations of Catalonian workers.



These observations can hardly detract, however, from the very substantial merits of this broadly conceived, extremely informative, and skillfully executed work that must be termed a first-rate scholarly labor.

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H. A. ENNO VAN GELDER, editor. *Gegevens betreffende roerend en onroerend bezit in de Nederlanden in de 16<sup>e</sup> eeuw* [Sources Concerning Real and Movable Property in the 16th-Century Netherlands]. Volume 2, *Industrie, vrije beroepen* [Industry and the Professions]. Rijks geschiedkundige publicatiën, Major Series, 141.) The Hague: Martinus Nijhoff. 1973. Pp. 408.

This second volume of inventories of real and movable property completes the final work of the Dutch historian H. A. Enno van Gelder. He died in 1973 at the age of 83 after a prolific life of scholarship that included editing the *Tijdschrift voor Geschiedenis*, which he helped to found. From beginning to end his attention was focused on the era of the Dutch revolt, which he interpreted with an unusual degree of emotional commitment.

The 264 inventories published in this two-volume work are intended to illustrate the nature of wealth holding among the various strata of the population of the Netherlands at the time of the revolt. The present volume includes inventories of men active in industry and the professions.

The volume is well edited. It includes footnotes that are very helpful in identifying people, clarifying antiquated terminology, and explaining pertinent legal matters. It also includes extensive indexes of all names, places, occupations, and religious institutions mentioned in the documents as well as an index of "special subjects," which identifies all references to such possessions as books, art objects, and shares in commercial ventures. Of special interest is a very complete inventory of the household possessions of a certain Oude Maria Dircksdr. It is thought that Rembrandt van Rijn was later raised in nearly identical surroundings. Equally interesting are the libraries of certain professional men, which are cataloged in full and thoroughly annotated.

One could go on; these records unveil a wealth of fascinating details. But certain objections to the basic conception of this project remain. The documents do not represent a random sample of those available, nor are they sufficiently numerous to form the basis for a

systematic analysis of wealth holdings. These two volumes of published inventories must stand as a treasure trove of details to illuminate the material culture of the Netherlands at the time of the revolt.

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G. J. SCHUTTE. *De Nederlandse Patriotten en de koloniën: Een onderzoek naar hun denkbeelden en optreden, 1770-1800* [The Netherlands Patriots and the Colonies: An Investigation into Their Ideas and Actions, 1770-1800]. (Historische Studies uitgegeven vanwege het Instituut voor Geschiedenis der Rijksuniversiteit te Utrecht, 29.) Groningen: H. D. Tjeenk Willink. 1974. Pp. 272. 30 gls.

This is a Utrecht University Ph.D. thesis, and it reads like one. That is to say, it is a thorough, conscientious, perceptive, and impartial investigation of its subject, with the monotony occasionally enlivened by humorous asides. It is unfortunate, as the author frankly admits, that the topic itself is of only marginal interest. The fate of the Dutch colonies was a relatively minor concern to the "patriots" of the 1770s and 1780s. When they finally came to power in 1795 and were faced with the problem of putting their "Enlightenment" ideas, which ranged from radical left to mildly reformist right, into practice, they were completely frustrated by English control of the maritime routes to the East and West Indies and by their own inability to send out a strong military and naval expedition to enforce what reforms they proposed. These were modest enough in all conscience, as was only to be expected in view of the inherently conservative Dutch character at that time and the danger of sparking slave rebellions in the West or Indonesian uprisings in the East. If the much-touted slogan "Liberty, Equality, and Fraternity" was put into practice for the indigenous inhabitants as well as for white officials, merchants, planters, and clerks. The Cape of Good Hope might have afforded an apparent exception, but it was seized by the English before any far-reaching reforms were implemented. The English summary (pp. 217-23) tells the average reader all he needs to know about this topic. The Dutch text can be skipped by all save the most committed connoisseurs of the history of the Enlightenment in all its aspects.

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c. SMIT, editor. *Bescheiden betreffende de buitenlandse politiek van Nederland, 1848-1919* [Documents Concerning the Foreign Policy of the Netherlands, 1848-1919]. Third Period, 1899-1919. Volume 6, *Buitenlandse bronnen, 1899-1914* [Foreign Sources, 1899-1914]; volume 7, *Buitenlandse bronnen, 1914-1917* [Foreign Sources, 1914-1917]; volume 8, *Buitenlandse bronnen, 1917-1919* [Foreign Sources, 1917-1919], part 1. (Rijks Geschiedkundige Publicatiën, Major Series, numbers 128, 137, 145.) The Hague: Martinus Nijhoff. 1968; 1971; 1973. Pp. xxvi, 718; xxvii, 589; xxxv, 670.

With these volumes Smit nears the end of his part of a massive project started a generation ago, that of making available all useful documents on Dutch foreign policy from the adoption of the constitution of 1848 to the end of the First World War. Smit's responsibility covered the 1899-1919 era. By 1965 he had edited five hefty volumes of documents from Dutch sources. These three final volumes (the second part of the last volume, summarized in Smit's introduction and containing American and Belgian documents from 1917 to 1919, should appear this year) add additional detail from English, German, French, Belgian, and American archives. As before, Smit has furnished impeccably edited volumes with extensive indexes and annotation. The documents deal with two eras in Dutch political history—from the Boer War to 1914, roughly the heyday of the Calvinist-Catholic coalition, and then the difficult days of the world war.

The prewar days were generally quiet. The pace slackens after some material relating to the aftermath of the Boer War—the English military attaché, albeit half-seriously, reports what he saw as the Dutchman's "inveterate and confirmed belief in his superiority. . . . The only way to argue with a Dutchman is to kill him, and it is also the only method he respects." The Belgians argue about coastal waterways and make sporadic attempts to strengthen ties with their fellow neutral. A great deal of interest and time is devoted, in particular by the German embassy, to speculation on the succession in case Wilhelmina should have no heir (the question became moot in 1909 with the birth of Juliana). Occasional pan-German propaganda concerned the English; anti-Krupp feeling bothered the Germans; the eventually abortive project of the fortifications for coastal defense at Flushing was of special interest to the military attachés. But, all in all, it was a leisurely time. By 1912 the English ambassador could not recollect in any of his long years of

service "any twelve months which has been so devoid in interest."

As the war broke out, Dutch sentiment was ambivalent. The German embassy noted with satisfaction the pro-German (or at least anti-English) feeling of many, including political leaders (Kuyper, Cort, Heemskerk); the English were sure that the mass of the people were anti-German. Both observations were correct. With very few exceptions the Dutch wished to remain neutral. Of the 1,159 pages devoted thus far to the war years in Smit's present volumes of documents, the great bulk deal with the increasingly difficult effort of the Netherlands to maintain neutrality. As the war went on, the ever-tightening English blockade aroused official evasion and public resentment, but this was overshadowed in public opinion by the desperate German submarine warfare. The documents show, in sometimes bewildering detail, the struggle to stay neutral despite what the Dutch saw as provocations from both sides.

As the war ended and the Kaiser fled to the Netherlands, a new era began. There is some interesting English comment on the uproar caused by Troelstra's demand for a socialist republic (Colijn is already seen as the coming strong man of the antisocialist parties). Yet the old persists, and Smit can end in mid-1919 on a leisurely note, as Dutch and Belgians argue about the Ghent-Terneuzen canal to the lower Scheldt.

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TAGE KAARSTED. *Indenrigsminister Ove Rodes dagbøger, 1914-1918* [The Diary of Ove Rode, Minister of the Interior, 1914-1918]. (Skrifter udgivet af Jysk Selskab for Historie, 29.) Aarhus: Universitetsforlaget. 1972. Pp. 229.

The start of the Great War presented the Danish government with a difficult set of problems. Ill-prepared militarily and caught between a natural antagonism toward Germany as a result of the war in Schleswig-Holstein and the geographical reality of close proximity to Germany, nonbelligerency offered the only reasonable alternative. But the exact nature of Denmark's neutrality had to be formulated with the accession of Germany and Great Britain. The Danes feared that Germany, intent upon closing the Baltic, would apply severe pressure. In addition, the leading military figures in Denmark believed that it was highly likely that Great Britain would attack if Denmark took any steps to mine the Great

Belt. The radical government of C. Th. Zahle tried to steer Danish policy around these problems and minimize the negative effects of the war on the development of the economy. The government on the whole was successful in both endeavors. The development of these policies is outlined in the diaries of the minister of the interior of the Zahle government, Ove Rode.

Rode was an important figure in the Zahle government, and his diaries illuminate a variety of problems Denmark faced during the war: the constitutional question, the Iceland issue, foreign policy, intraparty struggles, and the conservation and rationing programs. The latter topic, for which Rode was primarily responsible, is one of the most interesting themes of the diaries, but the problem is far too complex to be dealt with effectively in two hundred pages.

Kaarsted has done a good job of preparing the diary. It is relatively easy to follow the daily entries. The editor has not, however, tried to annotate the diary with comparisons from other primary or secondary sources. This shortcoming is unfortunate simply because Kaarsted has worked extensively on Rode and the period and is capable of giving the casual reader of the diaries a better understanding of the significance of the Rode material. The work will be of interest to specialists on modern Denmark.

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MICHAEL ROBERTS, editor. *Sweden's Age of Greatness, 1632-1718*. (Problems in Focus Series.) New York: St. Martin's Press, 1973. Pp. x, 314. \$14.95.

The Thirty Years' War added responsibilities on the Continent to Sweden's already complex military-diplomatic situation, and these essays focus on the attempts to meet these burdens in the period from Gustavus Adolphus to Charles XII. Gustavus Adolphus, a military and organizing genius, had taught how to "make war sustain war on foreign soil," but he could not provide for continued maintenance of armed strength in time of peace. Problems connected with this century-long struggle make up the substance of this volume.

Out of his own profound knowledge of the period the editor writes an introduction that sets the stage for a galaxy of Swedish scholarly talent. Sven Lundquist discusses "The Experience of Empire," including the administration of the conquered provinces that supplied man power and external bastions and a signif-

icant portion of the Swedish budget. Sven-Erik Åström examines the relationship of Sweden's economy to her role as a great power. Sweden lived largely on a natural economy, but her increasing complications abroad demanded cash. French subsidies helped but entailed unwanted obligations; duties levied in the Baltic ports were more satisfactory but did not survive military setbacks; the ultimate recourse was return to the system of "living off her own." This necessary but economically retrogressive policy was implemented by Charles XI through the *indelningsverk* (the allotment structure), a system by which both army and civil servants were paid in kind by the farms or districts allotted them. With this system were associated both the *reduktion*, by which the Crown recovered vast estates from the nobility, and also the achievement of absolute monarchy made possible by this weakening of the nobility. *Indelningsverk*, *reduktion*, and absolutism run like three red threads throughout the essays. Kurt Ågren in his chapter on the *reduktion* sweeps away many oversimplified generalizations about it, and Göran Rystad illustrates it, and much else, in his account of the spectacular career of Magnus Gabriel de la Gardie. Stellan Dahlgren is involved with all three topics in dealing with Charles X and the constitution and also in his chapter on estates and classes. Michael Roberts, on the Church, is interested in doctrine but analyzes, too, the Church's relation to royal absolutism and its vital role as agent for the state in the integration of the conquered Danish provinces into Swedish society. Alf Åberg has written a brief but illuminating piece, "The Swedish Army, from Lützen to Narva."

Roberts has provided the smooth-flowing translations and frequent cross-references. Perhaps in the preface he overemphasizes the uniqueness of these contributions for, under different headings, he has already handled in excellent fashion much of the same subject matter in his *Essays in Swedish History* (1967) and *Sweden as a Great Power: Government, Society, Foreign Policy* (1968). Nevertheless, on the topics here presented we have added insights and a welcome dimension of up-to-date Swedish scholarship.

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JOUKO TEPERI. *Arvon mekin ansaitsemme: Jaakko Juteinin aatemaailman eräät päälinjat* [We, Too, Merit Respect: Some Major Themes

in Jaakko Juteini's Ideological World]. (Historiallisia Tutkimuksia, 85.) Helsinki: Suomen Historiallinen Seura. 1972. Pp. 177.

Jaakko Juteini (Juden, 1781–1855), for twenty-six years a clerk of Viipuri's municipal council, has a modest niche in Finland's history as a writer (chiefly Finnish verse), linguist, and harbinger of Finnish nationalism. He has been called the first Fennoman.

Jouko Teperi's interesting survey of Juteini's major concerns provides a fuller ideological portrait than has been previously available. Unlike the Scandinavia-oriented Turku romantics of the early nineteenth century, Juteini welcomed the accession of Alexander I, whom he greatly admired, as Grand Duke of Finland in 1809. But in common with the Turku students, Juteini warmly espoused the cause of the Finnish language, long the object of discrimination by a Swedish-speaking elite. Mirroring the rationalism of eighteenth-century "utile dulci," Juteini's popular writings were "entertaining as well as useful, denouncing evil, advancing good, and singing the praises of a life of moderation." He sought to rescue his much-suffering countrymen from oft-recurring ailments, measles and malaria, and from human frailties, among them excessive drink and envy. His harshest words were hurled against self-seeking clergymen, dishonest judges, and incompetent officials.

Attacks on the established order invited retaliation, and Juteini was severely criticized in many quarters; one of his writings was publicly burned in Viipuri's market square. Not possessing a martyr's temperament Juteini lapsed into some sixteen years of discreet silence. Later in life he resumed his literary activities, and in 1840 Helsinki University awarded him an honorary doctorate.

Teperi's interpretation of Juteini's Weltanschauung is richly interspersed with selections of poetry and prose. There is a bibliography of archival and published sources, a German-language summary, and a complete catalog of Juteini's literary output.

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HELMUT BERDING. *Napoleonische Herrschafts- und Gesellschaftspolitik im Königreich Westfalen 1807–1813*. (Kritische Studien zur Geschichtswissenschaft, number 7.) Göttingen: Vandenhoeck & Ruprecht. 1973. Pp. 160. DM 28.

This brief, very informative study, which was inspired by recent French research on the

economic characteristics of Napoleonic society, investigates the emperor's practice of rewarding his subordinates with Crown domains carved out of the new Westphalian state. The extensive Westphalian archives, now in East Germany, supplemented by material in French and West German archives, enabled the author to analyze this policy in detail. The role that expropriation and other French innovations played in altering the corporative-agrarian structure of local society is more difficult to determine since statistics are sparse and the area's legal and economic arrangements were particularly complex. Berding uses quantitative analysis "whenever possible," but he combines it with broader political and social interpretations. His interests, in any case, go beyond the strictly local. The kingdom that Napoleon created for his brother Jérôme was intended to be a model state that would demonstrate the benefits of the emperor's system to all of Germany. At the same time, no conquered territory was subjected to more far-reaching expropriation. Events in Westphalia may thus offer insights into Napoleon's motives and the interaction of his policies in general.

Among the devices by which the emperor tried to legitimize his rule was the creation of a new hereditary nobility. He was sufficiently a man of the *ancien régime* to believe that the dignity of this *corps intermédiaire* between throne and people required not only titles and wealth but also estates, enjoying legal and tax privileges, settled inalienably on the family. The prohibition of entail in the *Code civil*—one of its more revolutionary articles—was easily amended, but the abolition of manorial land in France during the Revolution could hardly be reversed. Consequently Napoleon obtained what he needed in Italy and Germany. In Westphalia he claimed half of the domains of the dispossessed princes, entrusting the properties to commissioners not accountable to the Westphalian government. In vain Jérôme and his ministers fought the emperor's demands, which reduced the kingdom's revenues by nearly one-fourth and from the outset rendered solvency, orderly administration, and smooth relations between the empire and its model satellite out of the question. It was only one of the ironies of the situation that Napoleon's commissioners returned to the status quo ante by demanding that tenants and farm laborers fulfill even those obligations attached to the domains that the absolutist princes had let lapse.

In Westphalia, as elsewhere, economic policies

motivated by the emperor's foreign and domestic concerns often compromised legal and social modernization. The practice of rewarding followers with expropriated land at the expense of allied governments and the perpetuation of agrarian privileges no longer admissible in France were relatively minor but characteristic strands in the bundle of ambiguities that his rule bequeathed to subsequent German history.

PETER PARET  
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*Biographisches Wörterbuch zur deutschen Geschichte.* Founded by HELLMUTH RÖSSLER and GÜNTHER FRANZ. Second revised edition by KARL BOSL *et al.* Volume 1, A-H. Munich: Francke Verlag. [1973.] Pp. xii, 1266. 180 fr. S.

This, the second edition of the postwar biographical handbook of German history, is the work of three prominent historians of the Federal Republic. Both Karl Bosl, who was responsible for the excellent medieval entries, and Hanns Hubert Hofmann, on church history and national socialism, represent the post-1945 historiography while Günther Franz was renowned for his work on the Peasants' War (1525) even before World War II. Yet Franz's entries are, with some exceptions, perhaps the most Whig-liberal, while Hofmann's are often irresistibly directed to some rather narrow nationalist biases that reflect the persistence of old Prussian *étatisme*. In contrast, his entries on the Enlightenment bishops and archbishops of southern Germany are exemplary and include the best modern historiographical attitudes.

On the whole, however, the chief criterion of selection is based on the nineteenth-century tradition of judging historical importance primarily in terms of the development of the nation-state. It is the state's preservation and progress that thus justifies all policies of *raison d'état* to the exclusion, usually, of social, cultural, and humanistic values. Thus Hermann Göring's biography is nearly as long as Goethe's, and while Hegel is given his due, Husserl and Heidegger are omitted. Psychology cannot have been ignored, as Herbart is included, but Sigmund Freud is not. Jewish politicians, whether of the left or right, are not excluded and are objectively treated, however. Although Erhard is included and praised, Willy Brandt is not, and Adenauer is criticized for his patriarchal authoritarianism.

Although most of the entries are written in

a terse, objective style, it is in regard to the sensitive features in the interpretations of major political figures and cultural heroes that the lag in modern social science education in Germany makes itself felt. The categories are archaic, except in an existentialist sense, and date to the era before World War I. Thus we see demonic drives à la Max Weber, and late capitalism according to Sombart.

Some interpretations are so seriously problematical that they cannot be overlooked. Thus it is emphasized that Copernicus was of German "blood." As if this accounted for his achievements! Certainly Copernicus's family was quite loyal to the Polish Crown, and he himself led Polish troops against the Teutonic Knights. His father's ancestry seems less German in its ethnic origin than his mother's, and the family surname is in any case a Slavic one. Certainly the immediate cultural influence, perhaps the cosmopolitanism of Poland at the time, and the influence of his education in Renaissance Italy had more to do with his discoveries than the ethnic origins of his parents. It is also false to assume that Luther accepted Copernicus, for his literal interpretation of Scripture kept the sun where it stood when Joshua told it to stand still.

In keeping with the importance of the emerging national state, Frederick William the Great Elector is praised for all his policies, even for suppressing the local parliaments and their "outdated" interests. While Frederick William I is criticized for his harsh policies, his state building is given its usual place, and Frederick II has, of course, with Bismarck, one of the longest entries in the work. Frederick's achievements as a warrior are given lengthy analysis, and the second Silesian War is blamed on Maria Theresa's "irreconcilable" attitude. Although the harshness of his mercantilist policy is pointed out, the essential glorification of his economic aid to industry and agriculture shows an uncritical acceptance of the older historiography. The bibliographical essay at the end of the article is excellent, but Hofmann believes that "it is just the image of Frederick which has become the touchstone of national self-awareness and of the self-regulation of its historical picture."

Adolf Hitler has the longest entry in the work; it is also by Hofmann. The summary of research is excellent and rather factual. The final evaluation is viewed in terms of Hitler's terrorism and bloody sacrifices, his "conjuring up" of World War II, his loss of the eastern territories, and the division of Germany.

Of Göring, on the other hand, it is mentioned that at Nuremberg he defended himself and the German people with "courage." Such resistance figures as Carl Goerdeler and Dietrich Bonhoeffer are also given sympathetic treatment. The former, it is noted, was executed with a hand ax on the "special urging" of the prosecutor. The second volume is already in press.

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ALBRECHT LOBECK. *Das Hochstift Meissen im Zeitalter der Reformation bis zum Tode Herzog Heinrichs, 1541*. (Mitteldeutsche Forschungen, number 65.) Cologne: Böhlau Verlag. 1971. Pp. viii, 193. DM 28.

The coordination of the Church with the purposes of the secular state is a subject of abiding interest to Reformation scholars, but the story is by now so familiar in its outlines that only painstakingly detailed accounts of particular situations will tell us something new. This book, a dissertation completed in 1943 by a young theologian who died in the war a year later, now published with the aid of Heinrich Bornkamm, holds the magnifying glass to events in the bishopric of Meissen during a three-year period from 1539 to 1541 when Duke Henry the Pious of Albertine Saxony strove to bend bishop and diocese to his will. Following a helpful sketch of the bishopric's geography, economy, and institutions, Lobeck traces the typically intricate relationship of diocese and duchy, a source of constant friction turning on the incompatibility between the bishopric's legal status immediate to the empire and the wish of the dukes to make it over into a territorial subject. The struggle was turning in favor of the dukes long before the Lutheran Henry inherited the duchy from his Catholic brother George in 1539. With the right of presentation of all cathedral canons resting securely in their hands, the dukes in fact controlled the bishop elected by the canons.

The Reformation years 1539-41 saw the process of subordination completed. Efforts by the bishop to avert the catastrophe by meeting the Lutherans half way (lay cup, clerical marriage) failed. Two visitations in 1539 and 1540 made churches and monasteries in the bishopric conform to Saxon Lutheranism. In order to prevent the bishop from acting on his right to be represented at imperial diets, the duke closed all borders to the diocese, placing it under an economic interdict. The pressure proved irresistible. By 1541, while continuing

a tenuous *de jure* existence, the bishopric had in practice been *gleichgeschaltet*.

Lobeck's description of these events springs no surprises on us. But its meticulous and fair-minded consideration of all relevant issues and processes adds some welcome detail to our knowledge of political and religious operations in the early years of the Lutheran Reformation.

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MACK WALKER. *German Home Towns: Community, State, and General Estate, 1648-1871*. Ithaca: Cornell University Press. 1971. Pp. xi. 473. \$14.00.

This is an important and original work that fits into several categories: European urban history, the political history of Germany in the period from 1648 to 1871, and the history of the German political tradition. The home towns as defined by Walker constituted a special type of community, too large and complex to be called villages, too small to have developed a separate hierarchy of patrician class and citizens. They were characterized by a bewildering variety of constitutions but recognizable by their strong exclusiveness, walled off in a physical, legal, and psychological sense from the peasantry, nobility, free itinerant merchants, and journeymen, as well as the civil servants of the state bureaucracy. The circle of gild control of economy and usually politics, of intermarriage and social exclusiveness, and of stability over more than two centuries produced an inbred local culture with its distinct type of citizen, who differed somewhat from the resident of the larger German cities and greatly from the Western European bourgeois.

Walker traces the fate of these communities from the Peace of Westphalia to the abolition of the gild economy in the 1860s with the subsequent evolution of the home town into the modern small town. Specialists may wish to argue whether his definition adequately takes care of the distinction between territorial, residence, and free imperial towns. Given the legal variations between nearly every German town, definition is always dangerous and often impossible. Walker bases his work on a wide comparative survey of the literature from the seventeenth century to the present and on his own archival investigation of the Franconian town of Weissenburg from 1780 to 1825, which he uses to test and derive his hypotheses about



the towns in general. It could be argued that the attempt at synthesis is premature, that many more contemporary investigations of towns and cities (such as Gerald Soliday's recent study of Frankfurt) are necessary as a preliminary. Walker's hypotheses are stimulating, bold, and will probably be controversial, but they offer an insight hitherto lacking into the characteristics and origins of the German small burgher class in its uniqueness and historical development, which even illumines twentieth-century politics of the country towns. He offers ideas to which the specialist in German political, social, and urban history will have to respond as the dialectic of the problem proceeds.

ROGER WINES  
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EBERHARD WEIS. *Montgelas, 1759-1799: Zwischen Revolution und Reform*. Munich: C. H. Beck'sche Verlagsbuchhandlung, 1971. Pp. xv, 489. DM 65.

This is the first volume of a two-volume political biography of Montgelas, one of the founding fathers of the modern state of Bavaria. The present volume covers the first forty years of Montgelas's life. It deals with the unknown, formative years of Montgelas, not with his great reforms of the early nineteenth century.

It describes Montgelas's role as a book censor at Munich and his career as a *Legationsrat* at Zweibrücken and as political adviser of Duke Max of Zweibrücken, the future duke of Bavaria. The author sees in Montgelas a disciple of the Enlightenment and classical diplomacy, a liberal and cosmopolitan in the sense of the eighteenth century and an admirer of the French Revolution, and at the same time as a strong advocate of the sovereignty of the state, an *étatist*. Indeed the interests of the state of Bavaria dominated Montgelas's political thinking and activity even when he was only the political adviser of the duke of Zweibrücken. He pursued a most skillful diplomacy when the very existence of the state of Bavaria was threatened by Austria's plans of annexation. He insisted that the state of Bavaria be compensated for the territorial losses the Wittelsbach family had suffered as a result of the French annexations left of the Rhine River, preparing the acquisition of numerous ecclesiastical and secular states in Francia and Swabia. In the interest of the state, Montgelas also advocated at an early date the dissolution of the monasteries in

Bavaria. It is surprising to see that Montgelas was an unusually keen historian who resorted to ingenious historical and legal arguments to justify the dissolution of prince-bishoprics and monasteries. It was also during these early years that Montgelas formulated his program of a thorough reorganization and modernization of the administration of the state of Bavaria. The key ideas of his reform program are contained in a memorandum that he submitted as early as 1796.

The author describes in considerable detail conditions and events that were related to Montgelas's career, such as the secret order of the illuminati in Munich, the conditions of the duchy of Zweibrücken on the eve of the French invasion, and the beginning of the dissolution of monasteries in the last year of the reign of Duke Karl Theodor of Bavaria in 1799.

This first volume of Montgelas's biography constitutes a fine scholarly effort. It is based on an enormous mass of archival material that had never before been used. The work is extremely detailed and therefore of great interest to the specialist. Montgelas emerges as an unusually intelligent, versatile, and articulate statesman. But of course this first volume does not yet really establish his historical greatness. I am looking forward to the publication of the second volume, which will be devoted to Montgelas's great reforms in Bavaria.

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JOSEF MERGEN. *Die Auswanderungen aus den ehemals preussischen Teilen des Saarlandes im 19. Jahrhundert*. Volume 1, *Voraussetzungen und Grundmerkmale*. (Veröffentlichungen des Instituts für Landeskunde des Saarlandes, number 20.) Saarbrücken: [Saarbrücker Zeitung], 1973. Pp. 434. DM 30.

Josef Mergen, author of numerous specialized studies of emigration from various Saarland towns, here attempts a general survey of emigration from the Prussian Saarland in the nineteenth century. The emphasis of the work is on emigration to the United States, with brief treatment accorded emigration to Brazil, Algeria, Russian Poland, and France. About one-third of the book consists of documents, including such things as letters of emigrants and Prussian state regulations establishing the legal formalities of emigration. The main conclusion of Mergen's volume is that emigration was prompted largely by economic motives.

The Saarland, a densely populated area that experienced economic decline after its detachment from France in 1814–15, contained many small farmers, coal miners, and artisans who proved susceptible to the blandishments of agents and to the appeals of relatives and friends who had already emigrated. "To find a better existence" is a phrase encountered repeatedly in the documents quoted so freely by Mergen. To a lesser but still significant extent emigration occurred in order to evade military service. Virtually no one emigrated for religious or political reasons. Mergen also adds useful information on the work of agents, on government propaganda directed against the agents, on the economic impact of emigration, and on the sufferings of emigrants aboard ship and in their new homelands. In Poland and Algeria especially, economic conditions, climate, and general circumstances were so adverse as to impel the majority of surviving emigrants to return to the Saarland.

Although Mergen's organization is systematic, his method is not analytical. The book is chock-full of statistics, tables, and graphs, but these are left largely to speak for themselves, as are the lengthy quotations on virtually every page. There is no sustained analysis, indeed no sustained narrative. Many, if not most, of the quotations are essentially repetitious, and while some provide colorful detail, the impact is generally one of tedium. In compiling his quotations Mergen also failed to tap the enormous English-language literature on American immigration. An occasional quotation from James Truslow Adams—the only American historian cited—will not suffice. Better knowledge of conditions in America would have permitted a more critical understanding of the subject and perhaps have prevented such errors as referring to the District of Columbia as a state, to Minnesota as a town, and dating the Homestead Act in 1854 instead of 1862. The misdating leads Mergen into mistaken conjectures about reasons for emigration in the mid-1850s.

A subsequent volume will list all the emigrants by name and place of origin.

OTTO M. NELSON  
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CARL-AXEL GEMZELL. *Organization, Conflict, and Innovation: A Study of German Naval Strategic Planning, 1888–1940*. (Scandinavian University Books. Lund Studies in International History 4.) Stockholm: Esselte Studium. 1973. Pp. 448. 99.75 S. kr.

According to the author, the purpose of this study is twofold. First, and ostensibly most important, Gemzell hopes to contribute to a "fund of observations" about organization, conflict, and innovation that will provide the historian with a methodological framework for investigating modern bureaucratic institutions. His second task is to reconstruct the development of German naval strategic planning during the 1888–1940 period using the historical evidence either to confirm or modify existing social science theories.

The advantage of applying theories such as an organizational or conflict theory should outweigh the dangers of jargon and oversimplification. In my opinion, the value of this book lies less with Gemzell's attempt to provide a framework for combining social science methodology and historical research than the contribution he has made to our understanding of German naval planning. In many ways this book is a continuation of the author's 1965 study, *Raeder, Hitler und Skandinavien. Der Kampf für einen maritimen Operationsplan*, which documented the German naval planning against Scandinavia during the interwar period. In this present book Gemzell convincingly demonstrates the navy's interest in expansion northward as far back as the 1880s and reconstructs the importance of this idea for internal and external power struggles in four periods of naval strategic development: 1888–1914, 1914–18, 1918–39, and 1939–40. In each period the navy's strategic interest in Scandinavia centers around a combination of personal ambitions and rivalries among various military and political commands far beyond the navy's attempt to find a solution to its historical strategic weakness. In short, Gemzell reveals the continuity of naval planning in the era 1888–1940.

While other historians have commented on the intense struggles within the navy and the close relationship of naval planning to outside factors, Gemzell represents the first scholarly attempt to deal with naval planning as a whole. Herein lies the value and distinction of this work, and one hopes that the author will continue his studies into the period 1940–45.

In spite of the author's claims for his interdisciplinary comprehensive approach, there is still a great need for individual studies to find and describe other levels of conflict and relationships in the German navy's planning as, for example, the ties between industry and naval development. Thus Gemzell's criticism of recent monographs on German naval history

as being too narrow in their approach appears grossly unfair. It is understandable and even praiseworthy that Gemzell should try to avoid the overconcentration on the central, high-level decision matters. Yet the model approach that Gemzell favors seems to treat individuals as more or less predictable, responding in a like manner to similar stimuli. The historian of German naval history cannot, however, underestimate the significance of individual personalities such as Tirpitz and Raeder, who exercised such a strong influence on the navy and its planning, nor neglect the specific circumstances that brought and kept them in power.

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KLAUS ERICH POLLMANN. *Landesherrliches Kirchenregiment und soziale Frage: Der evangelische Oberkirchenrat der altpreussischen Landeskirche und die sozialpolitische Bewegung der Geistlichen nach 1890*. With a foreword by WALTER BUSSMANN. (Veröffentlichungen der Historischen Kommission zu Berlin, number 44.) Berlin: Walter de Gruyter. 1973. Pp. x, 329. DM 112.

This excellent but expensive monograph analyzes the social movement that swept along the Prussian Protestant clergy in the 1890s. Breaking new ground, the author lays bare the complex motivations of the *Oberkirchenrat*, the powerful administrative organ of the Old Prussian *Landeskirche*, by far the largest Protestant church in Germany. The *Oberkirchenrat* helped unleash the movement in 1890 only to work mightily first to contain and then dam up its political streams by way of a controversial decree in 1895 that discouraged further clerical involvement in social causes.

How does one explain such inconsistent behavior? The lesson conveyed by Pollmann is that the most obvious answer is not necessarily correct. Contemporaries and later historians assumed that the conservative state shaped the social-political policy of the *Oberkirchenrat*. After all, the Prussian *Landeskirche* submitted to state controls and to the authority of the king, its *summus episcopus*. The switch in church policy seemed to parallel closely the course of the state. Moreover, younger pastors, led by Friedrich Naumann, had begun to promote the cause of agricultural workers against conservative estate owners, whereas church and state officials alike expected the clergy to com-

bat subversive and irreligious socialism, not the existing order imperiled by socialists.

Pollmann draws a more complicated picture to show that the *Oberkirchenrat* did indeed pursue church, rather than state, interests in its decree. Neither the *summus episcopus* nor the General Synod denied the *Oberkirchenrat* sufficient authority to safeguard church interests as it perceived them. When clerical political involvement appeared to threaten church solidarity, the *Oberkirchenrat* did not hesitate to draw in the reins. In this sense, the Old Prussian *Landeskirche* hardly acted differently from free churches likewise limited in their social involvement by their constituencies. State ties did, however, enter the calculations of the *Oberkirchenrat*. Wilhelm II and church officials alike regarded the ruler's church government as essential to inducing harmony among contentious church-party factions. Chafing at this restraint, the church right agitated to shift authority from church government to synods, which the right dominated, and to dismantle state controls. Inadvertently the *Oberkirchenrat*'s initial promotion of the social movement intensified these aspirations. For a church courting alienated workers could not appear as a tool of a state that contested their emancipation. It was Adolf Stoecker who in the 1890s nailed the planks of church liberation and social action into an alluring program that would jeopardize the ruler's church government and with it church solidarity. The *Oberkirchenrat* wished to defuse a potentially explosive situation in the church long before it issued its decree.

Weighty as these arguments may seem, the parallel courses of church and state make it difficult to sort out possible state influences on the *Oberkirchenrat*'s policy. Pollmann is not able to assert that the ruler as *summus episcopus* exerted no pressure on the *Oberkirchenrat*, and he admits that church government could not long persist in a social policy at odds with the royal will. Finally, the quasi-religious reverence of church authorities for the monarchical order may have automatically kicked up quasi-religious defenses against any assault on it, however indirect. Pollmann has gone about as far as he can to vindicate his thesis but not far enough to remove reasonable doubts.

The decree itself brought no more than a Pyrrhic victory. For social-political activism within the clergy had begun to wane before the decree descended. In the long run, Pollmann contends, the decree's frustration of clerical social activism blunted social concern

among the clergy, induced political apathy, and probably intensified nationalism as a surrogate for social action.

The monograph fills a gap in the literature of the period by explaining how the realities of the institutional church conditioned the social and political activities of its clergy. But the thread of the argument is hard to follow. Needed throughout are low-level generalizations that would more effectively match up analyses of many related developments with the sweeping conclusions at the end.

DANIEL R. BORG  
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HOLGER H. HERWIG. *The German Naval Officer Corps: A Social and Political History, 1890-1918*. New York: Oxford University Press, 1973. Pp. xiv, 298. \$19.25.

Holger Herwig's well-organized and clearly written book concerning the development and limitations of the German naval officer corps provides historians with a welcome parallel to Martin Kitchen's *The German Officer Corps, 1890-1914* (1968). Past neglect of the sociological aspects of the German Imperial Navy has been corrected by means of topical treatments devoted to the birth of the battle fleet, the Kaiser's relationship to "his" navy, and the selection and training of officers and their relationship to one another. Drawing upon unexplored portions of the German Naval Archives, Herwig offers the reader an accurate and colorful picture of a service that contained the seed of greatness but failed to nurture it.

A product of the middle class and predominantly staffed by middle-class officers, the German navy long enjoyed a liberal and cosmopolitan reputation. During its growth from 250 officers in 1872 to over 3,000 in 1914, the navy lost this liberality and instead adopted the army's stultifying standards of conduct and honor as well as its reactionary attitudes toward democracy, liberalism, and Jews. The naval officer corps underwent a process of feudalization, which was so well described by Gordon Craig with respect to the German military.

As the technical requirements of a modern navy made engineer-officers indispensable, the *Seeoffiziere*, who regarded themselves as the only true command, segregated themselves from the "inferior" engineer-officers by means of rigid social criteria. Discrimination was even harsher with respect to the deck officers, who were treated as ratings. Each officer category strove to improve its own status and remained insensi-

tive to the aspirations of the group below it. The result was isolation from one another, from the sailors, and from the rest of society.

A direct line leads from the officers' preoccupation with status to the mutiny of 1917 and the revolution and mutiny of 1918, which are the primary concerns of the author with respect to the political machinations of the naval leadership. One omission mars an otherwise valuable account: the navy's puzzling failure to construct enough submarines. Wedded to the Dreadnought concept, the navy feared that a postwar Reichstag would confine its appropriations largely to submarines. An increase in the number of junior grade submarine officers would reduce the position and influence of the senior officer corps, and survival as a caste was accorded priority over the development of the only weapon that offered a chance of winning the war!

PETER W. BECKER  
University of South Carolina

DIRK HAGENER. *Radikale Schulreform zwischen Programmatik und Realität: Die schulpolitischen Kämpfe in Bremen vor dem Ersten Weltkrieg und in der Entstehungsphase der Weimarer Republik*. (Veröffentlichungen aus dem Staatsarchiv der Freien Hansestadt Bremen, number 39.) Bremen: Carl Schünemann Verlag, 1973. Pp. 255.

The social divisions in Germany during the century before 1914 are increasingly coming to be studied through the special prism afforded by the history of education. Until recently work in this area has concentrated upon the education of elites in the university and Gymnasium. Hagener directs his focus away from these institutions toward the specific problems of the *Volksschule* crisis in Bremen during the years before and after World War I.

A study of the Bremen crisis is welcome. It was here that problems evident in mass education throughout Wilhelminian Germany came to a head over an issue that had shaped political conflicts regarding elementary education for a century: the role of the churches and religion in the *Volksschule*. Bremen's problems began innocently enough. In 1905 the *Volksschule* teachers passed a resolution protesting the rigid conservatism of the city's chief school inspector (superintendent) on the question of religious instruction in the curriculum. Basing his narrative on meticulous research in the Bremen Staatsarchiv and on published sources, Hagener relates how the initial narrow conflict escalated

in the next years to the point where the traditional assumptions of teachers regarding German schools and society were called into question.

The struggle ended in 1910 in complete defeat for the reformers, but not before Bremen's teacher corps had been radicalized into adopting the revolutionary school program of the Social Democrats. The founding of the journal *Roland* to carry the message that radical social change must precede meaningful school reform highlighted the radicalization. Social change, or at least its possibility, came in 1918, and the second half of this study, less convincing than the first, treats efforts to implement the pre-war reform vision in the revolutionary context of postwar Bremen. These efforts, too, failed, in part as a consequence of factors outside Bremen's control; still, Hagener's failure even to discuss the possible impact of the war begs the question of continuity between the pre- and postwar situations, a question he himself poses.

The effort, as Hagener puts it, to examine the relationship between "education and society, pedagogy and politics, school reform and the reform of society," while welcome, is only partially successful. The introductory analysis of Bremen's social and political structure is not applied systematically to the analysis of the crisis itself. Instead, the clichés of social analysis—"bourgeois-capitalist society" or "bourgeois class state"—are made to carry the freight. The first section of the book dealing with the pre-war crisis is nonetheless an important contribution to the developing understanding of the relationship of education to the larger German society.

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JÜRGEN KOCKA. *Klassengesellschaft im Krieg: Deutsche Sozialgeschichte, 1914–1918*. (Kritische Studien zur Geschichtswissenschaft, number 8.) Göttingen: Vandenhoeck & Ruprecht, 1973. Pp. 230. DM 24.

Investigating class relationships in Germany during World War I, Dr. Kocka finds that, in spite of some counteracting tendencies, the rift between wage earners and industrial and agrarian employers became deeper and broader during the war. Conflicts of interest between the Junker and the leaders of industry became less important than they had been prior to 1914; the rivalry between the war industries, favored

by the government and the military, and the disadvantaged peace industries never reached a point at which they would have jeopardized entrepreneurial solidarity. Also, small business tended to follow the lead of the big employers. On the workers' side, the wage differentials between munition workers and labor in non-war industries failed to prevent them from taking a common attitude against prolongation of the war, restriction of civil liberties, and war profiteering and in favor of democratic reforms. The white-collar workers also moved toward a limited solidarity with manual labor. Thus the characteristic feature of German war society became aggravated dichotomy, which did much to set the scene for the revolution of 1918.

These propositions are hardly new and certainly not very startling, but Kocka has done a valuable service by documenting with extreme thoroughness the validity of these theses. In this respect his work is equal to that by Gerald Feldman, who deals with the same events from a different angle (*Army, Industry and Labor in Germany, 1914–1918* [1966]), and it is superior to Feldman's by a far better balance of judgment; Kocka painstakingly records the factors weakening class solidarity and in each case points out the reasons why nevertheless the forces of solidarity prevailed.

Although on the whole Kocka agrees with the Marxists on the importance of the class divisions in wartime Germany, he rejects the Marxist theorem that the state proved itself exclusively the instrument of the ruling class. Kocka makes a convincing case for the proposition that to some extent the state played an independent role because its very survival depended on its ability to satisfy the requirements of warfare. These requirements included a much higher degree of interference with business than industrial and agrarian entrepreneurs liked; they also included the limitation of working-class discontent through concessions. Although these concessions were too hesitant and too inconsistent to buy strong labor support, they did antagonize considerable sections of business.

All this is well explained by Kocka, and the explanation is abundantly substantiated. The only major objection that can be raised against his presentation is the almost total neglect of regional differences. At least the peculiar conditions in Bavaria deserved more attention than Kocka's brief references to South German anti-Prussianism.

Kocka, besides writing substantive history, also



regarded his book as a case study in using a sociological structure model for the analysis of historical events, thus making a contribution to historiographic methodology. The present fashion of sociohistoriography, however, should not obscure the fact that, since history is always dealing with society, no history ever written has failed to use sociological categories. To be sure, historians may gain new insights by using complex sociological models for their analysis. But Kocka's model, a society split by a deep rift between classes, is too simple for this purpose, and it does not seem to me that his methodological aspirations have reached their goal.

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HEINRICH POTTHOFF, editor. *Friedrich v. Berg als Chef des Geheimen Zivilkabinetts, 1918: Erinnerungen aus seinem Nachlass*. (Quellen zur Geschichte des Parlamentarismus und der politischen Parteien. First Series. Von der konstitutionellen Monarchie zur parlamentarischen Republik, number 7.) Düsseldorf: Droste Verlag. 1971. Pp. vi, 234. DM 58.

This seventh volume of *Quellen zur Geschichte des Parlamentarismus und der politischen Parteien* includes an essay on the historical role of the civilian privy council, a biographical sketch of Friedrich von Berg, and his reminiscences written in 1920. A few documents describe the functions of the council while others deal with Berg's influence as its chief. Von Berg was a Prussian civil servant of ultraconservative bent, a personal friend of the Kaiser, and an unswerving supporter of the Hindenburg-Ludendorff team. His intense Prussian loyalties made him suspicious of South Germans, a distrust fortified by strong anti-Catholic feelings. He resisted constitutional changes, strongly opposed democratization in Prussia, and endorsed only slight modifications of the three-class electoral system.

Although Berg understood the seriousness of Germany's position in 1918, he continued to believe in victory for the Central Powers. While four German offensives raged on the western front, Berg made plans of his own. His first goal was to effect a reconciliation between the Kaiser and Ludendorff. He succeeded at least in part, although the crises of October 1918 would again lead to sharp disputes between supreme war lord and first quartermaster general. Berg's second concern was to restore his

imperial master's self-confidence and involve him more in the conduct of affairs. He had no illusions about Wilhelm's capacities, but Berg believed that the prerogatives of the monarchy had to be reaffirmed. And so the Kaiser emerged from his villa at Spa to visit the front more often and give speeches at home. The latter was of dubious value, as Berg should have known, despite his efforts to control Wilhelm's oratory.

The best illustrations of Berg's political influence and the power of his office are the intrigues he launched against the chancellor. In September 1918 Berg tried to replace Count Hertling with a military dictator, but this was too much even for Hindenburg and Ludendorff. The chief of the civilian privy council was responsible to the emperor only and could exert influence without control by the constitutional authorities. He had to contend with the chiefs of the military and navy councils, but it appears from Berg's account that in 1918 he had the field largely to himself. The navy chief, Müller, was in disfavor, while General Lyncker, the military chief, was ill and soon to retire. This shift to the civilian chief is the most interesting clue we find in these reminiscences; most of the political details have long since been known. The Kaiser is his usual unstable self: assuring Emperor Karl in February that Germany could wage war for two or three more years, while in September he shed tears in telling Berg that since the war was lost he would be held accountable for the bloodshed, the widows, and the orphans.

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RICHARD GRUNBERGER. *Red Rising in Bavaria*. New York: St. Martin's Press. 1973. Pp. 164. \$9.95.

Mr. Grunberger's book is a sympathetic treatment of the series of revolutions in Bavaria in 1918-19. It begins with the background of the uprising of November 7, 1918, discusses Kurt Eisner's life before the Revolution, and then proceeds to a description of events until the suppression of the *Räterepublik* by the armed forces of the Reich and the Hoffmann government at the beginning of May 1919.

This book has little to offer the professional historian. It does nothing that has not been done far better in Allan Mitchell's *Revolution in Bavaria, 1918-1919* (1965), and it has many flaws. Grunberger, to judge by internal evidence, seems to lean rather heavily on Mitchell.

But since neither bibliography nor footnotes are provided, it is very hard to assess some of the statements made or to allocate the responsibility for errors. Certainly the author has drawn on some sources a good deal less reliable than flamboyant.

Imprecision of language sometimes blurs or twists meanings. Bavaria, for instance, was scarcely a "province" either in the German or the American meaning of the term. Officers and units are described as belonging to the *Reichswehr* before there was a *Reichswehr*. In general, military events are particularly poorly handled, with many factual errors appearing in even the simplest matters and important distinctions are ignored. Even the old National Socialist myth of the significance of "Freikorps Epp" in the campaign against Munich is perpetuated. "White" atrocities seem considerably exaggerated.

Eisner is presented as being more consistent than he was in fact. For example, no mention is made of his brief enthusiasm for the war in 1914, and his vacillations in office seem underplayed, although they cost him the support of even such an old friend and fellow pacifist as Friedrich Wilhelm Foerster. Still, the simple, factual presentation of the series of revolutions is the best feature of the book and is generally reliable, although not all readers will agree with the tone or the value judgments, especially regarding the SPD and elements to their right.

Even though his basic attitudes are clear, it is rather difficult to grasp the author's specific thesis. His final reference to the experiment in council rule having been a short step on the long road toward renewal of the earth is especially obscure, since he precedes it with strong criticism of the revolutionists and does not tell us what his renewal conception means.

Grunberger writes smoothly and well, although the relevance and meaning of some vignettes are not clear. Interesting presentation, smooth transitions, and effective use of dramatic scenes will add to the book's appeal to the layman. It is light and readable popular history, going smoothly over rough ground, but providing pitfalls for the unwary.

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JÜRGEN HEUER. *Zur politischen, sozialen und ökonomischen Problematik der Volksabstimmungen in Schleswig 1920.* (Beiträge zur Sozial- und Wirtschaftsgeschichte, number 5.) Kiel: Walter G. Mührlau. 1973. Pp. xvi, 238. DM 20.

Central Europe has been plagued by ethnically impure frontiers, repressive minority policies, and irredentism. A half century of Prussian centralization and Germanization failed to eradicate the Danish-speaking minority of North Schleswig. As part of the Versailles peace settlement the victorious Allies decreed a plebiscite for the area in 1920. Allied opposition to direct Danish-German negotiations and the voting procedures the Allies imposed (an en-bloc vote in two large zones and a simple majority decision) embittered the Germans and left a substantial German-speaking minority north of the new frontier.

Jürgen Heuer's Kiel dissertation concerns the internal history of the plebiscite: the background and consequences from the point of view of the German-speaking Schleswigers. Although the work is based entirely on published sources, it is detailed, probing, analytical, and generally free of the nationalistic prejudice that even today mars much German writing on the Schleswig question.

Heuer describes the sharp social cleavage—whose existence antedated the war—between the middle classes and the workers in Schleswig. After 1918 the Social Democrats and the bourgeois parties each sought to organize the German population in opposition to Danish and/or Allied demands, but the cleavage prevented more than temporary cooperation between them. The cleavage was seen particularly in the bourgeois exclusion of socialists from local government and in sharply varying degrees of willingness or unwillingness to compromise with Danish demands. The cleavage continued after 1920 as bourgeois politicians sought a revision of the frontier, whereas socialists worked for German-Danish reconciliation on the basis of the new status quo.

Heuer is at his best in the analysis of "*Grenzland* economic history": he shows that the frontier of 1864 had created a particular pattern of production and marketing, which the new frontier disrupted, and that substantial governmental investment after 1920 was unable to restore the former prosperity on the German side.

Yet Heuer attempts less than one might have hoped for. Although he suggests the importance of economic motives in the plebiscite, he does not use quantitative or mapping techniques to analyze the vote in terms of economic and linguistic factors. He cites very few Danish works, and these in German translation; he is only barely aware that the Danes themselves were divided on the extent of their demands.

He fails to speculate on the significance of the plebiscite for the subsequent shift of Schleswig voters from liberalism to nazism. The definitive internal history of the plebiscite remains to be written.

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REINHARD SCHIFFERS. *Elemente direkter Demokratie im Weimarer Regierungssystem*. (Beiträge zur Geschichte des Parlamentarismus und der politischen Parteien, number 40.) Düsseldorf: Droste Verlag. 1971. Pp. 323. DM 48.

The history of the Weimar Republic is filled with political institutions that were either aborted or distorted, and a book like the one under discussion can only serve to lend weight to the argument that the republic never really got off the ground as an effectively functioning political enterprise. This was not entirely the consequence of divisions over its legitimacy or over the kinds of political institutions that should dominate its political life. The French Third Republic had a very inauspicious beginning and suffered from severe internal conflicts and contradictory institutions, but it had sufficient time to overcome these problems at least to the point where its institutions assumed a stability of purpose and function and were employed in predictable ways. Weimar, in contrast, was ruled by decree in many important realms throughout its existence, and the relationships between the parliamentary and direct democratic components of its constitutional structure were never satisfactorily adjusted.

Dr. Schiffers has written a valuable study of the major institutions embodying direct democracy in the Weimar Republic: the direct election of the president by the people, the referendum, and the initiative. His book makes a useful contribution in at least two respects. First, Schiffers provides important new information on the origins of the constitutional provisions for the referendum and initiative. He notes that Germany had to look to Switzerland and the United States for models since she had none of her own. His discussion of the influence of the prominent liberal jurist from Baden, Julius Curtius, and of other South German constitutional thinkers and political leaders upon the deliberations in Weimar, as well as his analysis of the views of various political leaders and legal experts concerning the referendum and initiative, should prove interesting to students of constitutional history. Second, he provides a very full discussion of the debates and dis-

cussions concerning the direct election of the president as well as an account of the referendums and initiatives proposed or held during the republic's existence. Historians have paid little attention to many of these initiatives, and Schiffers does a fine job of demonstrating the way in which not only political parties, but even more significantly, middle-class interest groups and paramilitary groups sought to use the instruments of direct democracy for their own purposes.

It is unfortunate that Schiffers's book is not very lively, in part because the material itself is somewhat dry, in part because the author does not do enough to make his material more appealing and give it greater scope. Nevertheless, the work is solid and the conclusions are sound. Schiffers argues that the elements of direct democracy in Weimar were ambivalent in both their origins and their application. While partially inspired by the desire to give the system a more democratic character, the initiative and referendum and the direct election of the president were also promoted by forces wishing to constrain the parliament. Similarly, antiparliamentary forces used the referendum, as in the case of the Young Plan, to undermine the republic. Schiffers convincingly argues, however, that even in less spectacular instances the use of the referendum and initiative, or the mere threat to use them, served to increase political tensions and thwart the effectiveness of parliamentary and other governmental institutions whose strengthening might have made the republic more stable.

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HELMUT HIRSCH. *Experiment in Demokratie: Zur Geschichte der Weimarer Republik*. [Wuppertal:] Peter Hammer Verlag. 1972. Pp. 184. DM 15.

KARLHEINZ DEDERKE. *Reich und Republik: Deutschland 1917-1933*. (Klett Studienbücher.) 2d ed.; Stuttgart: Ernst Klett Verlag. 1973. Pp. xii, 316.

MARTIN SCHUMACHER. *Mittelstandsfront und Republik: Die Wirtschaftspartei—Reichspartei des deutschen Mittelstandes, 1919-1933*. (Beiträge zur Geschichte des Parlamentarismus und der politischen Parteien, number 44.) Düsseldorf: Droste Verlag. 1972. Pp. 271. DM 54.

The three books under review have little in common except that they deal with the Weimar Republic. Hirsch's study consists of ten slight interpretive essays on political personalities who

influenced the history of the republic. They are not strictly biographical sketches but rather discursive and reflective comments on aspects of the period that the author associates with certain individuals. The ten essays range from "Bürgerliche Hoffnungen: Woodrow Wilson" to "Das Ende vom Anfang des braunen Systems: Adolf Hitler." In between are impressionistic treatments of Rosa Luxemburg, Friedrich Ebert, Hans von Seeckt, Walther Rathenau, Hugo Stinnes, Gustav Stresemann, Owen D. Young, and Heinrich Brüning. Although they appear to be based on a systematic reading of many of the best books on the period, one often feels that they might have appeared in the *feuilleton* section of a Weimar newspaper of liberal persuasion. This work is not likely to attract the specialist or have any value for him. It was apparently written for the German general reader of serious paperbacks. In view of this, it is curious that in the bibliography of seventy-two items, seventy are in English, two in French, and none in German!

The second edition of Karlheinz Dederke's textbook, by contrast, offers something for the specialist, the student, and the general reader. It is an admirable synthesis of the many facets of political, economic, social, and intellectual history of the period. It is well written, with brief and pointed quotations and excellent statistical and graphic material to support the text. The author is conversant with the most recent interpretations and the historiographical debates, but he is not afraid to reach controversial conclusions in areas where the debate among historians continues. The work with which it could be compared is Albert Schwarz, *Die Weimarer Republik* (vol. 4, pt. 3 of the *Handbuch der deutschen Geschichte* [Constance, Akademische Verlagsgesellschaft Athenaion; 1958]). Although it is intended to be used as a textbook, it is written on the *Handbuch* model. It can serve as a work of quick reference as well as a general introduction to the period. It makes most of the older political histories of the period seem old-fashioned by comparison. Indeed, it is one of the few German works I have read recently that warrants a translation.

Martin Schumacher's monograph on the *Wirtschaftspartei* should be read in conjunction with a volume he edited earlier, *Erinnerungen und Dokumente von Joh. Victor Brecht 1914-1933* (Düsseldorf, Droste Verlag; 1970), which was reviewed in this journal (77 [1972]: 541-42). Brecht was a leader of the *Wirtschaftspartei* and one of the few men in the party who ap-

parently left papers or wrote an autobiography. He also contributed a patina of "class" to this "green grocer's party." He was a university professor while the other leaders were master bankers and brewers, officials of tax payers' organizations, drugstore owners, and pub and hotel proprietors.

The *Wirtschaftspartei* changed its name in 1925 to the *Reichspartei des deutschen Mittelstandes*, but the change did not affect the goals of the party or its significance. The party never succeeded in arraying all the various *Mittelstand* organizations behind it. Nor did it produce a *Sammlung* of middle-class parties, one of its goals in the period from 1928 to 1930. The *Wirtschaftspartei* remained a splinter party of very narrow interests. It was largely concerned with protecting the self-employed artisans and small businessmen of Germany from socialism, government controls, and taxes that were harmful to the "little man." Pierre Poujade would have felt at home in it.

From its founding the *Wirtschaftspartei* was closely associated with an important pressure group, the *Haus- und Grundbesitzervereine*, which scholars have previously neglected. A great variety of *Mittelstand* protective associations and artisan groups also intermittently supported the party. It reached its peak at the national level in 1928, when it won 4.5 per cent of the total vote (1,397,000 votes). Its strength was in heavily Protestant urban areas, especially in Saxony. In 1928, for example, it won over 10 per cent of the vote in Chemnitz-Zwickau.

Schumacher's study of the organization of the party is excellent, but many questions about the *Wirtschaftspartei* and its electorate remain unanswered. The issues that concerned its voters are not adequately discussed. We do not really learn why the *Mittelstand* abandoned the liberal parties for the *Wirtschaftspartei*, nor do we fully understand, despite the frequent use of Theodor Geiger's phrase, "Panik im Mittelstand," why and how the electorate of the party became Nazi voters. They were conservative, nationalistic, moderately anti-Semitic, anti-feminist, anti-Marxist, and one might assume they would have drifted to the right, but the particulars of the plight of the *Mittelstand* and its radicalization do not apparently concern Schumacher. There are also serious organizational flaws in the book, which also hamper its effectiveness. Most of the forty-four volumes of the *Beiträge zur Geschichte des Parlamentarismus und der politischen Parteien* series have been dull, and this one is no exception. The editors of the series seem to prefer works with-

out personalities or human drama, but to me the result is desiccated history. Schumacher's study is "useful" because it fills a void, but that is faint praise.

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GEOFFREY PRIDHAM. *Hitler's Rise to Power: The Nazi Movement in Bavaria, 1923-1933*. New York: Harper and Row. 1973. Pp. xvi, 380. \$13.00.

JEREMY NOAKES. *The Nazi Party in Lower Saxony, 1921-1933*. (Oxford Historical Monographs.) New York: Oxford University Press. 1971. Pp. xvi, 273, 3 maps. \$13.75.

MAX H. KELE. *Nazis and Workers: National Socialist Appeals to German Labor, 1919-1933*. Chapel Hill: University of North Carolina Press. 1972. Pp. ix, 243. \$9.50.

HEINRICH AUGUST WINKLER. *Mittelstand, Demokratie und Nationalsozialismus: Die politische Entwicklung von Handwerk und Kleinhandel in der Weimarer Republik*. (Studien-Bibliothek.) [Cologne:] Kiepenheuer & Witsch. 1972. Pp. 307. DM 34.

Although these four books differ markedly in their methodologies, thematic focuses, and interpretations, all are concerned at least in part with varying aspects of the same subject: the sociopolitical role of the German *Mittelstand* in the twentieth century and the causal relationship between this social group and the rise of the Nazis to power. Pridham's and Noakes's works are essentially regional analyses of the Nationalsozialistische Deutsche Arbeiterpartei (NSDAP) itself. *Hitler's Rise* details the party's development in Bavaria, and *The Nazi Party* covers much the same ground for the area of Lower Saxony. Kele and Winkler seek to fill a larger canvas. Max Kele hopes to show that the "workers' party" component of the NSDAP's title was more than a cynical and propagandistic façade, and Heinrich August Winkler attempts to define the role of the *Mittelstand* in the sociopolitical history of Germany between the later years of the empire and the Nazi seizure of power.

The study of local or regional aspects of a national political movement is often useful in correcting the picture presented by documentation provenanced in the group's national office. In the case of the Nazi party it has been suggested that regional analyses would help to destroy the self-serving image of rigid centralization put out by the functionaries at the party's

Munich headquarters. Actually, Pridham's and Noakes's contributions tend rather to confirm the centralized nature of the NSDAP's operations before 1933. Both books underscore the importance of Hitler as the personal embodiment of the Nazi program, the propagandistic about-face that followed the national elections of 1928, and the importance of the party's social and economic affiliate organizations after the onset of the depression. To be sure, each adds useful refinements to the picture. Pridham demonstrates that the Catholic Church was roused to its anti-Nazi stand not so much by the results of the Nazi victories in the local and state elections of 1929 as by Alfred Rosenberg's paganistic writings. Both authors are able to put the political significance of the Nazis' anti-Semitism into perspective: the party was politically successful not because it was anti-Semitic per se, but because it integrated the anti-Semitic themes into its overall economic propaganda.

Largely because he chooses a more variegated region for his subject, Noakes makes contributions to the overall study of nazism that are somewhat greater than Pridham's. Noakes is particularly forceful in differentiating the spirit and atmosphere of the northern *völkische* movements from their southern counterparts in the years before 1923 and in documenting the reverse correlation between the Nazis' voting appeal and their participation in state governments. Once confronted with the necessity of dealing with the depression in an executive role, the Nazis' multifaceted propaganda approach of promising relief to all social groups turned against them and led to a noticeable vote decline. I have no real criticism of Noakes's substantive findings, though I think his conclusions might have been more differentiated if he had been able to incorporate to a greater extent the relevant literature appearing since 1966. Pridham's book makes less of an impact, largely because his findings can do little but confirm what was known before—albeit in lesser detail.

The books by Kele and Winkler are revisionist and thesis-oriented; both intend to correct a major "given" in the historiography of nazism. Briefly put, Kele contends that previous scholarship has unduly ignored the significance of the working-class element in the NSDAP's membership and voting appeal. The author attributes this failure at least in part to what he calls a liberal and Marxist bias among most historians. To buttress his thesis Kele points out that when the objective economic



gap between blue- and white-collar workers (*Arbeiter und Angestellte*) faded in the years after 1923, the Nazis then took advantage of this fact and forged a political union of the two social groups. In terms of organization, the working-class emphasis in the NSDAP found its culmination in the establishment of the NS-Betriebszellenorganisation (NSBO), an organization that Kele sees as a genuine union-like affiliate of the Nazi party. (There is a grain of truth in this, but Kele ignores that it was precisely for this reason that the NSBO met with undisguised distrust in many of the *Gaus*. Noakes points out that in Lower Saxony the NSBO was regarded with considerable suspicion by the majority of party members precisely because it seemed to introduce blue-collar elements into the party structure.) By 1931, according to Kele, the success of the Nazi effort to attract working-class support can be demonstrated with statistical data: about fifty per cent of the NSDAP's membership consisted of white- and blue-collar workers.

It is unfortunate that Kele's thesis relies upon a number of fuzzy conceptualizations and terminological sleights of hand. The most important of these is the term "worker." Kele readily acknowledges that the blue-collar element of the Nazi party remained small. Workers become a significant part of the NSDAP only when the term is broadened to include, in addition to blue-collar workers, white-collar employees, agricultural laborers, and employed artisans. That is to say, when the category is enlarged to encompass virtually everyone who was an employee (*Arbeitnehmer*). The semantic confusion reaches the point of distortion in the case of the artisans. To be sure, the objective difference between an assembly-line worker and a *Meister*-artisan employed in the same factory may have narrowed in the 1920s, but to ignore the subjective chasm between the consciously proletarian assembly-line worker and the artisan with his *Meisterbrief* and the *Mittelstand* mentality means that the researcher fails to recognize what the Nazis effectively exploited.

There are lesser problems as well. The author's attempt to correlate officeholders and ideological positions among the NSDAP's functionaries is not convincing. To begin with, left and right are not always clear in the Nazi context. Kele is convinced that Goebbels was ideologically left-wing. An equally good case could be made for his being simply a cynical opportunist. Above all, however, it did not really matter. It was the organizational and political genius of Hitler precisely to be able

to ignore ideological positions because the loyalty of the individual to him, Hitler, at all times superseded whatever ideological leanings the individual might have had.

Winkler's study of the German *Mittelstand* is both the most ambitious and the most significant of the four books under review. The work has a twofold aim: to present a structural analysis of the aims and composition of the German *Mittelstand* in the twentieth century and to revise the previously accepted view of the relationship between this social group and the rise of the Nazis. (A small caveat before proceeding to an analysis of the book's overall findings: the author deliberately presents his analysis in a neo-Marxist framework. He begins with the largely a priori judgment that the *Mittelstand* in the twentieth century is an obsolete social grouping; it has been objectively bypassed in the linear progress of history.)

The author begins his analysis with what he calls the feudalization of the *Mittelstand* in the Wilhelminian empire. The *Mittelstand* enjoyed a protected position after it recovered from the depression of the 1870s, limited only by the government's unwillingness to allow these sections of the economy to benefit at the expense of heavy industry or East Elbian agriculture. Consumer interests were, however, deliberately sacrificed to the demands of small retailers and artisans. With the Revolution of 1918, the *Mittelstand* understandably feared for its protected role, though, as Winkler makes clear, the fear was groundless. The republic failed to isolate the essentially reactionary elements of the old *Mittelstand*. Instead, the republican governments quickly abandoned whatever radical structural changes had been hesitatingly begun in the first weeks after the revolution, and by 1919 or so the *Mittelstand* felt strong enough to demand a continuation of the Wilhelminian protection policies, a cry for help that remained unabated throughout the life of the republic. Since the *Mittelstand* was aware that its interests ran counter to the majority of the population, it rapidly turned against parliamentary rule and sought to achieve its goals first through executive action and later through a variety of antirepublican and anti-parliamentary political representations. In addition, the *Mittelstand* stressed its role as a *Stand* that provided the basis of a viable alternative to the rule of the masses in parliament.

Winkler shows with painstaking care that the Nazis were merely the last stage in the *Mittelstand's* search for political support of their demand for economic protectionism. The

*Mittelstand* began by supporting the Deutsche Demokratische Partei (DDP), feeling that this was the safest guarantee against the dangers of socialism. When the DDP not only cooperated with the Socialists but stressed its support of parliamentarism as well, the *Mittelstand* drifted to the Deutschnationale Volkspartei (DNVP). After the Nationalists' sellout in 1924, the artisans and retailers hoped that the *Wirtschaftspartei* would become a decisive factor in the Reichstag, and when that, too, proved to be an illusion, there remained only the Nazis. Hence the *Mittelstand's* massive support for the Nazis was in no way a revolutionary act but simply the last stage in a long line of retrenching efforts to save the protected position that the group had inherited from the Wilhelminian empire.

I find the thesis very persuasive, though, as Winkler himself notes, there are a number of regional variations in this pattern that will have to be explored more fully. As far as Bavaria and Lower Saxony are concerned, Pridham's and Noakes's findings in fact confirm Winkler's conclusions.

Finally, these four books suggest something about the needed direction of research in twentieth-century German social and political history. In terms of the history of the NSDAP itself, we may have reached the point of diminishing returns. There is, however, a continuing need for in-depth studies of significant German social groups, studies that do more than pose the question of the proto-Nazi elements in a group's value system make-up. Here, too, Winkler's study is exemplary: by incorporating the Nazi-*Mittelstand* relationship into the overall analysis of the group's development since the 1880s, the author is able to reduce the Nazi segment of the story to the shortest part of his book.

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MARTA L. DOSA. *Libraries in the Political Scene*. (Contributions in Librarianship and Information Science, number 7.) Westport, Conn.: Greenwood Press. 1974. Pp. xv, 226. \$12.50.

The title of this book is somewhat misleading. It is not a study of the role of libraries or librarians as active forces in the political scene. Rather it is a readable and well-documented study of Georg Leyh, a distinguished scholar-librarian who served as director of the Tübingen University Library from 1921 to 1947, and how he responded to the political pressure

exerted on German libraries and the library science profession during these years.

Georg Leyh, like many other members of the German university community, believed that a clear separation had to be maintained between scholarship and politics. This was the established pattern of Imperial Germany, where Leyh was educated, and he successfully continued it through the Weimar period. After 1933, however, he was unable to prevent the "coordination" of the libraries. His efforts during the following twelve years were devoted to trying to protect library collections from confiscation by the Nazis and, in wartime, from destruction by Allied bombing. After the collapse of the Third Reich, Leyh tried to re-establish "the detached objectivity of scholarship" in the library profession. Such a task was hopeless because almost immediately the growing division of the country forced librarians in the two Germanies to identify with the prevailing ideology.

Although the book, with its lengthy sections on the problems and development of German librarianship, will prove of more value to students of library science, historians can read it with profit, especially the two chapters on the Third Reich. Here it is demonstrated once again that the Nazi regime, despite its awesome power, was pervaded by a bureaucratic inefficiency brought by the jurisdictional disputes of competing state and party agencies. In the confusion that resulted, it was possible for determined individuals to protect a cherished interest, as Georg Leyh did with his library's major holdings.

The book raises one obvious question: what were the political views of German librarians? Leyh's attitude toward the Weimar Republic is not discussed, and other than his hostility toward the Nazi's interference in the world of scholarship, we can only surmise that because of his conservatism he was repulsed by Nazi barbarism. In every case the author has treated Leyh's response to political events solely in terms of his concern for the fate of German libraries and librarianship. Perhaps that in itself explains why so many German scholars failed to meet the challenge posed by the Nazis during the late Weimar years.

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FRANKLIN H. LITTELL and HUBERT G. LOCKE, editors. *The German Church Struggle and the Holocaust*. Detroit: Wayne State University Press. 1974. Pp. 327. \$15.95.

After Hitler's rise to power the Christian churches of Germany, whose general attitude to National Socialist ideology had been rather cool, attempted to find a *modus vivendi*. The dimensions of Hitler's religious nihilism were not immediately apparent. In his speeches as well as in *Mein Kampf*, Hitler on occasion had expressed a certain admiration not only for the hierarchical organization of the Catholic Church but also for the uncompromising stance in dogma of the Christian church in general. What many people failed to grasp was that this admiration at no time amounted to an approval of Christian beliefs.

Furthermore, a rapprochement between the Catholic Church and the new regime appeared possible with the signing of the concordat with the Vatican in July 1933, which granted the Church impressive concessions in matters of education. It was soon realized that the new official educational philosophy, based heavily on a "blood and soil" ideology and on Rosenberg's "mythus," would erode and erase the influence of the Church within the school. To attract public attention to this situation, Bishop Galen of Münster began the outright struggle with party and government when he prohibited Catholics from attending meetings sponsored by Rosenberg.

If the concordat at first appeared to augur a peaceful relationship between the Catholics and Hitler, it was the authoritarian-conservative tradition of the Protestants that seemingly would provide a basis for cooperation with the regime. The initial convergence soon came to a halt when Hitler and his "German Christians" successfully endorsed Müller for the position of *Reichsbischof* against Bodelschwingh, the candidate of the majority. The result was that the Protestant activists, especially around Pastor Martin Niemöller, formed the Confessional Synod at Barmen in May 1934. In time Niemöller and some of his supporters as well as numerous Catholic leaders were sent to concentration camps.

In this volume the struggle of the German Church against the National Socialist government provides the framework and background for the holocaust, the steps taken for the liquidation of German (and European) Jewry. In many respects the book is not only an indictment of the Church but also a tribute to those who early recognized the totalitarian threat and stepped into the breach.

The book consists of sixteen papers read before an international scholars' conference at

Detroit in 1970. The papers are grouped into sections that indicate the scope of the investigation: historical background, political considerations, theological implications, and personal reflections. In content they attempt to explore the past and assess the present. The moderator of the conference, Franklin H. Littell, is a recognized authority in the field. His paper, "Church Struggle and the Holocaust," also provides the title for the book. Outlining the events, Littell states that their "lessons cast long shadows across the present state of religion and politics in the United States."

The papers form an impressive whole but are too varied in content to permit individual review. John S. Conway points out the present state of research and the location of numerous sources (the archive of Kerrl's Ministry for Church Affairs is in Potsdam). Wilhelm Niemöller, brother to Martin Niemöller and himself a theologian, offers some detailed insights into the struggle of the Confessional Church. Michael D. Ryan, in his theological political analysis of *Mein Kampf*, traces Hitler's anti-Semitism to his immature and uncritical Vienna years, with the result that "when he thought that he had discovered a great sickness in society, namely, the presence of Jews, he fell into the greatest illness of his society, Antisemitism."

In such a wide presentation it is not surprising that the participants at times arrive at different conclusions. Beate Ruhm von Oppen maintains that the "mainspring and machinery" for the holocaust was not Christian but "post-Christian or neopagan." Henry Friedlander, on the other hand, suggests that Nazi anti-Semitism may be seen as a modern variant "of an older Christian theme." Finally, Richard L. Rubenstein, in a most provocative (in the sense of *anregend*) paper, by implication rejects both schools of thought and sees in nazism a "kind of Judeo-Christian heresy," lacking the true "pagan virtues of moderation and respect for limits."

This diffused presentation of the subject provides one of the chief merits of the book. Professors Littell and Locke and their associates deserve credit for the publication of these papers marked by scholarship and new insights.

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WILLY BRANDT. *In Exile: Essays, Reflections, and Letters, 1933-1947*. Translated from the German by R. W. LAST. Biographical introduction by TERENCE PRITTE. Philadelphia: University of Pennsylvania Press. 1971. Pp. 264. \$9.50.

The German edition of this collection of some of Willy Brandt's early writings was originally published in 1966 under the title *Draussen*. The selections, which are drawn largely from published articles and books, are organized topically into seven sections, ranging from "After the War—What Next?" and "For and Against 'The Other Germany,'" to "Spain" and "Norway." Brandt himself has provided brief introductions for each section as well as a special foreword for the English edition. A very sketchy biographical introduction has been supplied by Brandt's most recent English biographer, Terence Prittie.

Although the final choice of the materials included was made by the editor of the German edition, Günther Struve, Brandt offered his own suggestions, and the book does not wholly escape the apologetic tone one would expect to find in such a collection published with the cooperation of an active politician. Nevertheless, the inclusion of excerpts from some of Brandt's relatively radical early writings as well as some very strong statements about personages, such as Francisco Franco, who are still politically important explains why Brandt greeted Struve's selections "not . . . without some apprehension" and helps to alleviate the suspicion that it was designed simply to serve Brandt's immediate political interests.

The book suffers, however, from a number of other significant limitations. Because so few letters are included, and because even these are largely political in content, little insight is provided into the development of Brandt's personality under the pressures of exile and world war. Moreover, some of the selections, notably a substantial excerpt from a book published in 1948 about Norway under German occupation, contribute little or nothing to our understanding of either Brandt's personal experiences or his political views.

This book is, thus, not likely to be very useful to scholars in the field of modern European history. Nevertheless, for those who are simply interested in sampling the early work of this great statesman, it can be recommended as good and at times even gripping reading.

KENNETH R. CALKINS  
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RUDOLF STEIGER. *Panzertaktik im Spiegel deutscher Kriegstagebücher 1939 bis 1941*. (Einzelschriften zur militärischen Geschichte des Zweiten Weltkrieges, 12.) Freiburg: Verlag Rombach, 1973. Pp. 204.

Since 1967 the Office for Research in Military History of the Federal Republic of Germany has sponsored a paperback series relating to the war of 1939–45 that is separate from its own broader historical studies. In this twelfth work a young Swiss historian, who is also a tank officer, examines conceptions and practices in German tank tactics from 1939 through 1941.

Using mainly war diaries of armored divisions, corps, and some higher commands, which are now held in the military archives at Freiburg im Breisgau, but also extensive published materials and studies, the author underlines failures to assess the practices that brought victory in Poland in 1939 and in France and the Low Countries in 1940. After a general survey of activities of armored forces from 1939 through 1941 he analyzes aspects of tank tactics in ten brief chapters and emphasizes the familiar importance of cooperation with aircraft and also of relations with other arms and units. Loss of records for the Polish campaign through wartime bombing of the Potsdam archives did not keep him from finding materials for many points. War diaries for 1941 dramatically detail disasters from that "dogma of the battle of annihilation" which an Israeli officer, Jehuda L. Wallach, searchingly scrutinized in an Oxford historical study published in Germany, *Das Dogma der Vernichtungsschlacht* (1967–70), and which Larry H. Addington's *Blitzkrieg Era* (1971) also surveyed. Steiger sketches in detail the painful results from gross exaggeration of German capacities by the generals and Hitler and from disregard at once of the effectiveness of Russian forces and equipment and also of the problems that terrain, transportation networks, and weather presented. He thus adds evidence to Albert Seaton's *Russo-German War 1941–1945* (1971) and Barry Leach's recent notable *German Strategy against Russia 1939–1941* (1973). Details illustrate the superiority of the Russian T-34 tanks and also the failure to use them with major effectiveness in 1941. The author tellingly indicates how Russian generalship and fighting men as well as the mud, the winter, and the insufficiency of supplies and planning halted the German invaders and imposed defeat. There is a usefully extensive bibliography but no index. The informative appendixes include a survey taken on March 12, 1942, from the Fourth Panzer Division (in Guderian's Second Panzer Army) on that unit's experiences in 1941, including the fact that after impossible higher demands only fifteen per cent of their tanks were usable



by December 5 before the beginning of the Russian counterattack.

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HEINRICH SCHUUR *et al.* *Führungsprobleme der Marine im Zweiten Weltkrieg.* (Einzelschriften zur militärischen Geschichte des Zweiten Weltkrieges, 13.) Freiburg: Verlag Rombach. 1973. Pp. 150.

Military history can be as intellectually stimulating and as broadly informative as any other genre of historical writing. But one would never know it from reading this little volume, which consists of three aridly technical and narrowly focused studies produced by a trio of naval officers enrolled in an advanced training course at the Bundeswehr's Führungsakademie.

The book, as its title suggests, emphasizes problems of operational command. Each essay examines a single naval engagement, one in which the Germans either failed to attain their objective or came off second best in the encounter, and analyzes some of the tactical and operational lessons that can be learned from it. More specifically, the authors, drawing heavily on unpublished military and naval records, attempt to identify the salient causes of the German failure in each case. Heinrich Schuur exposes flaws in the planning and execution of Operation Juno, the abortive German naval effort in June 1940 to destroy a fleet of British transports evacuating Allied forces from northern Norway. Rolf Martens probes inadequacies in the German defensive response to Operation Chariot, the British Commando raid on the port of St. Nazaire in March 1942 that disabled the only dry dock on the Atlantic Coast big enough to take the German battleship *Tirpitz*. And Wolfgang Koehler highlights shortcomings in communications and combat intelligence during Operation Ostfront, in which the German navy lost the formidable battle-cruiser *Scharnhorst* in a spectacularly unsuccessful attempt to intercept an important British supply convoy en route to the Russian port of Murmansk.

Within the limitations imposed by their assignments, the authors have done their work with skill and care. Exhaustively researched, fastidiously documented, tightly organized, crisply written, and amply buttressed with maps, diagrams, and charts, these three studies are models of their kind. But therein lies the rub. For these narrowly technical essays, produced to fulfill a military-academic requirement

and designed to train naval officers in the exercise of their profession, have little or nothing to say to the academic scholar. The main outlines of the naval operations they treat are already well known, while the technical details they add are largely irrelevant—except perhaps to a handful of naval historians.

Several of the earlier volumes in this series have made a notable contribution to our understanding of World War II; this book, however, is unlikely to add more than a footnote or two to the military history of that epochal event.

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HERBERT SCHWARZWÄLDER. *Bremen und Nordwestdeutschland am Kriegsende 1945.* Volume 2, *Der britische Vorstoss an die Weser.* (Bremer Veröffentlichungen zur Zeitgeschichte, number 6.) Bremen: Carl Schünemann Verlag. 1973. Pp. 225.

This second volume of *Bremen and Northwest Germany at the War's End* takes up on March 23, 1945, with the opening of the British Twenty-first Army Group's offensive across the Rhine. After ranging back a week or so to cover Field Marshal Sir Bernard L. Montgomery's somewhat elaborate preparations for that event and the German First Parachute Army's effort to counter Montgomery's thirty-five British, Canadian, and United States divisions with ten understrength German divisions, the focus moves east 140 miles to Bremen. For that city, the largest in Montgomery's path across north Germany except for Hamburg 60 miles farther east, the moment of truth was approaching.

The first of what now appears will be three volumes suffers from some lack of definition. The enemy was only remotely on the scene; hence, the preparations to meet him had a vague busywork aspect about them. The present volume has no such weakness, although at the end Bremen is still virtually untouched, aside from the air raids that had long before become a part of its way of life. This book is, in fact, an absorbing and possibly unique account of the war as it was seen from the other side. It is remarkable that, from that viewpoint, one has almost a sensation of being transported out of the twentieth century and back to the Thirty Years' War.

Between the Rhine and the Weser the British occupy one town after another and on April 9 come within artillery range of Bremen when



they take Kirchweyhe. For the German civilians the trick seems to be to get the white flags out in time hopefully to placate the enemy without getting arrested or hanged as a defeatist by a last-minute German patrol. That is what the people of Bremen would like to do, too, but Bremen is different. Hitler has designated the city a "fortified place," and under Hitler stands the Commanding General Northwest, Field Marshal Ernst Busch, who in 1944, out of loyalty to Hitler, let his army group be all but destroyed in Russia. Busch has appointed General Fritz Becker combat commander for Bremen without giving him any troops. Becker, a somewhat ersatz Prussian of the old school, longs for just one of the divisions he once commanded on the eastern front, but the last of those is surrounded and being destroyed at Danzig. Nevertheless, for Becker "orders are orders." Some of the prominent citizens think of bribing or shooting him. One even makes a halfhearted try at the latter. Meanwhile, British artillery shells are dropping in Bremen, and rumor has it that *Gauleiter* Karl Kaufmann of Hamburg is going to bring his *Volkssturm* over to help defend Bremen—and establish an excuse for not having to do the same at Hamburg.

The British, for their part, are in no tremendous hurry, and they begin to move east across the Weser south of Bremen. On April 20 they fire leaflets into the city demanding a surrender within twenty-four hours. At that point the *Gauleiter* of Weser-Ems, Paul Wegener, who by shilly-shallying in the past has become a last hope of those who want to spare the city, declares he will do nothing. After that the chief of police, Johannes Schroers, who would like also to keep his job under the occupation, sends out a police sergeant to try to explain to the enemy that Bremen wants to surrender but cannot. The rest will follow in volume 3.

EARL F. ZIEMKE  
*University of Georgia*

ERNST-OTTO CZEMPIEL. *Macht und Kompromiss: Die Beziehungen der Bundesrepublik Deutschland zu den Vereinten Nationen 1956-1970*. (Krieg und Frieden: Beiträge zu Grundproblemen der internationalen Politik.) [Düsseldorf:] Bertelsmann Universitätsverlag, 1971. Pp. 189. DM 28.

Professor Czempel deserves high marks for the candor with which he discusses the role the Federal Republic of Germany has played in

the United Nations prior to its admission as a full-fledged member in 1973. Czempel points out that essentially that role has been shaped by West Germany's determination to block the recognition of East Germany as a separate sovereign state. Most of Bonn's appeals to the UN were connected with the *Deutschland-Frage*—calls for the investigation of violations of human rights or free elections in the German Democratic Republic (GDR)—just as in the UN's specialized agencies Bonn's representatives bestirred themselves more vigorously on behalf of that issue than on the special concerns of these agencies.

In these latter organizations to which West Germany has belonged since the 1950s, Bonn's efforts were directed primarily at blocking the admission of the GDR. Admission would have implied recognition; to the Federal Republic, which claimed to be the sole legitimate representative of all of Germany, this was unacceptable. As long as pro-Western nations had safe majorities in the agencies, this policy was fairly easy to implement. As Czempel reports, on some occasions the GDR was barred on grounds that were ludicrous. Gradually, however, the majorities began dwindling—a fact that contributed to West Germany's eventual decision to grant recognition to the GDR before it would be outflanked.

Meanwhile Bonn paid its own price for barring East Germany. In some cases it made especially large financial contributions to some UN agencies to make up for the losses resulting from the nonmembership of the GDR. On other occasions sizable payments were made to impress on these organizations the serious financial setbacks they would suffer if Bonn were to withdraw because of the admission of East Germany. In the UN itself Bonn refrained from applying for full membership since this could be had only in return for East Germany's simultaneous admission.

Chapters dealing with the West German parliament and public opinion indicate that few West Germans saw more in the UN than a means to promote the country's interests and policies. In this, of course, they did not notably differ from other nations. Czempel feels, however, that in pursuing its intransigent anti-GDR course West Germany proved even less amenable to compromise, the existential foundation of the UN, than the average UN member. With the recognition of East Germany no longer an issue, this may now, however, change.

ANDREAS DORPALEN  
*Ohio State University*

HANS HAUTMANN and RUDOLF KROPP. *Die österreichische Arbeiterbewegung vom Vormärz bis 1945: Sozialökonomische Ursprünge ihrer Ideologie und Politik*. With a foreword by KARL R. STADLER. (Schriftenreihe des Ludwig-Boltzmann-Instituts für Geschichte der Arbeiterbewegung, 4.) [Vienna:] Europa Verlag. 1974. Pp. 214.

The authors originally planned a synthesis of the history of the labor movement in Austria and of Austrian economic history as recommended by Victor Adler in 1906. They concluded that only a parallel description of the two developments up to 1945 was possible. The result is a terse and well-organized handbook, replete with valuable graphs and tables and accessible to worker as well as to academician.

Their early chapters summarize in traditional fashion the great influence of the banks and of railway construction in industrial development and the story of the boom that ended in the crash of 1873. In delineating the state's efforts to surmount the crisis, they seem to approve of the imperial assumption of most of the railways and are complimentary to Taaffe's social legislation. When they turn to the factional disputes among putative labor leaders of the 1880s, they are critical of Josef Peukert and the terrorism he inspired. They esteem Victor Adler for his unifying efforts (1886-89), but they are markedly detached in evaluating his later contributions. In dealing with the destructive effect of national rivalries upon the workers' movement, they are disapproving of Karl Renner and Otto Bauer for sponsoring solutions by administrative reforms "from above." They also place principal blame for the Czech secession upon the Austrian German wing, which should have been the most strenuous advocate of national freedom.

Judgments on the party's reaction to the coming of World War I are quite explicit. Social Democratic parliamentary figures and the party's bureaucrats and functionaries lived on the movement, not for it. Less convincing is their opinion that, thanks to pacifistic propaganda, there was no trace of enthusiasm for the war among the workers. Friedrich Adler's assassination of Stürgkh, they believe, was an asset for the party, for the masses regarded the act as a heroic gesture in behalf of peace. In response to pressure from rank and file, the party turned leftward to such a degree that the Communists never attracted a considerable following after the war.

The tragic story of the postwar quarrel between Social Democrats and Christian Social forces unfolds with clarity and reserve, though

the authors sympathize with the former. They assert that the workers did not lose their readiness to fight for the party's objectives. Rather, the leaders were too ready to compromise. The party's worst error was the failure to call a general strike on March 15, 1933, when Dollfuss sent police to occupy parliament. The civil war and German occupation that followed were the inevitable results of poor leadership, and the lack of a class-oriented, combative party meant that the workers were but puppets in the hands of the Nazi rulers.

WILLIAM A. JENKS

Washington and Lee University

BENJAMIN R. BARBER. *The Death of Communal Liberty: A History of Freedom in a Swiss Mountain Canton*. Princeton: Princeton University Press. 1974. Pp. xii, 302. \$12.50.

The author of this book is critical of what he calls the Western Anglo-Saxon liberal democratic thought and parochial political tradition. According to him they equate freedom with the absence of external constraints and democracy with elite rule sanctioned by the people through the device of representation. He finds that the Swiss mountain canton of Graubünden presents an enlightening alternative to this stereotype since its communities maintained until very recently a form of direct self-government far older than liberal democracy and completely unassimilable by its categories.

Barber thus has an axe to grind. A major difficulty he encounters is the fact that even such a small geographic and political entity as Graubünden is too diversified and complicated in its ethnic composition and history to lend itself to the kind of generalization he attempts. Thus he gets bogged down in a dry, general account of hundreds of years of Graubünden history, a subject fraught with difficulties of interpretation and obstructed by the infighting of local experts. Nevertheless, that part of his book has a certain quaint charm.

But, of course, Graubünden is used only as a foil against which the author grinds his axe (or is it the other way around?). While he is describing the workings of the "face-to-face community" as it is supposed to have existed in olden times he constantly looks ahead to its disappearance in our time because of the "inevitability and potency of pluralistic centralism in an industrializing society." The analysis culminates in the dire prediction that "the eclipse of communalism may mean for the Swiss the eclipse of political man as an auton-

omous participant in the self-governing of his collective life."

One wonders why Barber engages in such an unrelieved Cassandra song of pessimism and denunciation of what he calls the liberal European "growth mania." Ever so often he presents a good, accurately descriptive passage on the virtues of direct democracy as practiced in Switzerland. But such passages are invariably spoiled by the author's strong bias that is demonstrated in a devastating way in the few illustrations he selected for his book. On the last page of the text, for example, we see against the backdrop of a mountain chapel and glistening silvery glaciers the picture of a gray-bearded mountaineer walking in a religious procession, hands on his back, holding a rosary with a crucifix. The caption reads "Direct Democracy." Beneath it there is a photograph of a city street demonstration with three policemen in summer uniforms struggling with a young man, who apparently has just been hosed down, while a sullen crowd is watching on. Caption: "Representative Democracy." This is absurd.

The text is marred by many factual inaccuracies and errors, and so-called printing errors abound in English and German words.

HEINZ K. MEIER

Old Dominion University

ALFREDO BONADEO. *Corruption, Conflict, and Power in the Works and Times of Niccolò Machiavelli*. (University of California Publications in Modern Philology, 108. Published under the auspices of the Center for Medieval and Renaissance Studies, University of California, Los Angeles.) Berkeley and Los Angeles: University of California Press. 1973. Pp. vii, 127.

PETER E. BONDANELLA. *Machiavelli and the Art of Renaissance History*. Detroit: Wayne State University Press. 1973. Pp. 186. \$10.95.

Specialists in Italian literature, Alfredo Bonadeo of the University of California, Santa Barbara, and Peter Bondanella of Indiana University have contributed to dispelling the myth of Machiavelli. The thinker who emerges from their careful examinations of the texts within the appropriate historical contexts is far from being a disinterested "scientific" thinker, an immoral advocate of tyranny, or an exponent of a tragic human condition. He is instead an impassioned literary artist, a dedicated republican, and an optimist convinced of the possibility of social reform.

Professor Bonadeo has provided a much-

needed analysis of the Florentine's ideas of corruption, conflict, and power and their mutual relationships. Civic corruption arises with the decline of religious belief, the loss of a simple, nonluxurious way of life, the emergence of wealth, great disparities between rich and poor, and the concentration of power in one or a few. Always beginning in the ruling elite, corruption then spreads throughout the body politic unless it is curtailed by forceful measures. Republican Rome prior to the Gracchi is Machiavelli's ideal uncorrupt state, while Florence typifies corruption. The absence or presence of corruption is closely linked with the kind of conflict occurring in the state. Conflict in the Roman Republic was the consequence of a healthy division between patricians and plebs within the framework of law and the common good; or conflict may be productive of constant disorder resulting from self-seeking partisanship and factionalism within the elite of a corrupt society like Florence. Bonadeo stresses that for Machiavelli the main responsibility for corruption and factionalism always rests upon a specific social group: the wealthy, powerful, and noble. With these considerations Machiavelli's approach to power can better be appreciated. Good laws and institutions in Italy may not succeed in stemming corruption and factionalism without a powerful political actor, a "new prince" who for the redemption of a people (unlike Borgia) will use violence to subdue the divisive *grandi*.

In contrast to Bonadeo's sociopolitical work, Professor Bondanella offers a literary treatment of the continuity and development of Machiavelli's art of portraying historical and fictional characters. Inspired particularly by classical biography, history, and fiction, Machiavelli uses exempla in all his writings as an integral part of his argument to persuade and convince his readers, often distorting and changing historical fact to suit his argument. The zenith of his art of character portrayal is *The Life of Castruccio Castracani of Lucca* (1520) and *The History of Florence* (1525). Bondanella argues that the former is not an idealized biography or a preliminary for the later history, but a deliberate reshaping of an actual life to depict the archetypal prince. The portrait of a villain, Walter of Brienne, duke of Athens, is the high point of the latter book. To the modern reader with the novel in mind, Machiavelli's protagonists may appear psychologically simplistic, mere hollow stereotypes. However, he cannot be so judged, any more than can Homer or Virgil. Like them, he is attempting to reveal the per-

sonalities of his characters through their actions.

Apart from questions of detail, each author can be criticized for not going far enough. Bonadeo profitably might have related Machiavelli's views on corruption and conflict to his "conception" of human nature and to the position of the ancients, dwelt more upon Machiavelli's perception of the Roman experience, and connected more explicitly his strictures against the *grandi* with the actual political situation in Florence; and Bondanella might have been less reluctant to discuss Machiavelli's social and political thought in terms of his art of character portrayal. Nevertheless, both have performed useful services, and it is hoped that in the future they will elaborate upon these studies. Bondanella's emphasis upon Machiavelli's portrayal of the character of the Germans, the humble origins of Castruccio, his vignette in *The History of Florence* of Michele di Lando, the lowly leader of the Ciompi in 1378, the critical characterization of many of the *grandi*, including Lorenzo de' Medici, and the devastating picture of the French nobleman, Walter of Brienne, all seem to confirm Bonadeo's conclusions about Machiavelli's devotion to the principle of civic equality and steadfast opposition to the *grandi*. Clearly Machiavelli's thinking was forged in the crucible of political strife in Florence from 1494 to 1512 between the *grandi* and the *popolo* (or more properly *mezzani*) to whom he was deeply committed, despite his aristocratic lineage. Without this partisanship he would not have become a great political theorist.

NEAL WOOD  
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EDWARD R. TANNENBAUM and EMILIANA P. NOETHER, editors. *Modern Italy: A Topical History since 1861*. New York: New York University Press. 1974. Pp. xxix, 395. \$12.50.

It is difficult to know at whom this topical history is aimed, and it is even harder to decide to whom it should be recommended. Certainly not to the general public, for *haute vulgarisation* is neither the purpose nor the forte of these authors, and the general reader in search of an introduction to the subject can still do no better than H. Stuart Hughes's admirable little book, *The United States and Italy* (1953, 1965). To students, perhaps? But students on what level? Graduate students would be better advised to seek their instruction in the books from which these essays are derived, and under-

graduates to whom the volume or portions of it will be assigned will find the going heavy and for the most part unrewarding. To teachers of Italian history, then? To be sure, they will want to mine the book for its conveniently assembled facts and ideas, but they should be forewarned that if they have been following the literature on modern Italy with any constancy, they will take away from their labor a sense of frustration and a powerful impression of *déjà vu*.

There are a few exceptions. Denis Mack Smith has contributed an essay on regionalism that displays his usual combination of wit, wide-ranging knowledge of the sources, and unyielding hostility to the leaders of liberal Italy. While recognizing all the dangers of corruption and disunity that decentralization held in the conditions of late nineteenth-century Italy, he still concludes that regional government was the only course that could lead to the political education of the masses and the unblocking of parliament from local affairs getting in the way of the nation's most pressing business. Raymond Grew deserves praise for having tried to assess, in an intelligent and open-minded fashion, the many-sided impact of Catholicism on Italian life; and Rosario Romeo's essay, "Germany and Italian Intellectual Life from Unity to the First World War," though brief, is long and suggestive enough to make one realize that Italy is worthy of a detailed study of the sort that Claude Digeon devoted to France.

Without question, however, the most stimulating pages in the collection are found in Alberto Aquarone's closing commentary, "Problems of Democracy and the Quest for Identity." Aquarone takes issue with those who insist on viewing liberal Italy exclusively in terms of its failures and pleads for an approach to fascism that would be synchronic and alert to context rather than obsessed with origins. What needs explanation, he argues, is not why the liberal regime collapsed, but why it was established in the first place and how it managed to sustain itself so long in circumstances that were, to say the least, unfavorable. After seeing author after author make the usual references to the nefarious effects of the Southern Question on Italian life, it is refreshing to find someone who has the candor to point out that all modern nations have been industrialized by elites at the expense of exploited groups, whether inside or outside their own country, and then add that, seen in perspective, it was the north that lost and the south that gained from unification be-



cause the north has paid the price of the south's political and economic backwardness, as many northerners feared at the time of unification. Aquarone's theme is the Italians' continuing "quest for identity through some kind of dimly conceived and childishly pursued process of total regeneration of the country" (p. 373). The First World War and the Bolshevik Revolution fed flames to this longing; fascism was inspired by it and would be incomprehensible without it; and given the fact that the first partial integration of the Italian masses into the state occurred under Mussolini's regime, Aquarone finds it all the more amazing that the nation emerged from the experience as a liberal democracy, of which now, twenty-five years later, the Italians have reason to be proud. If Aquarone is unable to view the future with the same equanimity with which he views the past, it is because he fears that the interests and aspirations of the working class have ceased to coincide with the struggle for freedom and democracy. A debatable opinion, perhaps, but one that it is impossible not to find troubling.

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BERNARD D. WEINRYB. *The Jews of Poland: A Social and Economic History of the Jewish Community in Poland from 1100 to 1800*. Philadelphia: Jewish Publication Society of America; distrib. by Random House, New York. 1973. Pp. xvi, 424. \$10.00.

The author has devoted many years of his creative life and writing to the history and sociology of the Jews in Poland as well as in the United States, Israel, and Germany. His work, *Neueste Wirtschaftsgeschichte der Juden in Russland und Polen*, published in 1934 and recently reissued in Hildesheim, has been an outstanding contribution to this field. He has also edited a number of important documents, particularly from the Jewish community of Poznan.

In the present volume Dr. Weinryb offers a noteworthy review of the social and economic history of Poland from its early beginnings around 1100 to the postpartition period of 1800. It is a well-documented survey of the social history, taken in its broader sense to include many aspects of legal and intellectual history. In fact, the last three of the twelve chapters in the book are entirely devoted to important religious and intellectual movements of the

period from 1648 to 1800. The economic history, however, appears somewhat neglected. After a chapter devoted to the medieval period, when the Jewish communities were still very small, the economic aspects of the vital last three centuries are more incidentally treated in connection with occupational rights and other political, legal, and intellectual developments. This is particularly regrettable, since general Polish historiography of the last quarter of a century—when contributions to the more specialized field of Jewish history have greatly diminished as a result of the Nazi holocaust—paid particular attention to various phases of the economic life of the country. Thus one may have to disagree, for example, with the author's contention that by the end of the Middle Ages the total Jewish population of Poland amounted to no more than 10,000 to 12,000 souls, that it increased fifteenfold between 1500 and 1648, and that despite the great loss of more than one-quarter of its number during the catastrophic years of 1648 to 1667, it increased again (together with Lithuania) to some 750,000 in the following hundred years (pp. 114–16, 308–20, appendix 3). It should also be mentioned that the author's decision to lump together his survey of the three centuries from 1500 to 1800 (for which most of the information cited is derived from sources dated after 1648)—the distinguishing marks between, first, the earlier period of rapid growth during the sixteenth-century's golden age of both Poland and its Jewry; second, their subsequent destinies after the onset of the Counter Reformation under Sigismund III and Wladislaw IV; and, third, the general decline of the Polish-Lithuanian Commonwealth after 1648—appears considerably blurred. But these flaws do not seriously impinge on the high quality of the volume, which offers an eminently readable and documented survey of the history of a community destined to become the chief matrix of world Jewry in the nineteenth and twentieth centuries.

SALO W. BARON  
Columbia University

JOSEPH MACEK. *Jean Hus et les traditions husrites (XV<sup>e</sup>–XIX<sup>e</sup> siècles)*. (Civilisations et mentalités.) Paris: Plon. 1973. Pp. 390.

Joseph Macek has written a remarkable and erudite book about Jan Hus and the effects of Hussite history and myth for five centuries after Hus perished at the stake in Constance



in 1415. Macek stresses the Czech nationalism of Hus, his desire to have the Bible translated into Czech, and his special interest in the cultivated bourgeoisie of Prague.

Macek sees the Hussite movement as part of the "crisis of feudalism" that was causing a political and social effervescence at the end of the Middle Ages. By what authority do you command people to obey? That is the fundamental question in any political order. Hus was moving toward a sort of political Donatism, according to Macek. Those who held authority in church and in the secular government ought to be morally worthy of their charge. "It is evil to claim that a man who is superior because of his rank cannot be accused by the subject who is superior to him in the moral sense!"

Macek stresses the role of the imitation of Christ in the theology of Hus—a quality that clearly sets Hus off from the reforming thought of Martin Luther a century after Hus had died. Macek finds the program of Hus to be superior to that of Luther. "The model created by the first Reformation [that of Hus] concerns not merely the interior reformation of man, but all society. It places a much greater stress on the ecumenism of Christians and religious toleration."

But if Hus was tolerant, his followers were not. The Hussites showed a savage propensity to violence both against one another and against their general foes, the Roman Catholics. Many believed that Christ would shortly return to earth to devour his enemies. It seemed quite logical to radical Hussites to begin this process of merciless destruction in anticipation of the vengeful return of their divine Savior. The radicals were largely countrymen and detested the finery of the self-indulgent bourgeoisie of Prague. In the end John Zizka (whose skin was *not* made into a drum when he died) took the side of the rich and swept the poor from the field, burning alive seventy-five radical leaders. Sporadic violence continued, and in time the name "Hussite" became a synonym for anarchy.

Macek treats in detail the way later Czechs looked at Hus. Was he a saint or a villain? The problem remained acute as long as the superannuated Habsburg monarchy ruled the Czech lands. Writers who mentioned Hus favorably were likely to have their works censored, and Macek gives many examples. But a national movement like that of the Czechs—having few great historical personages—was bound to receive Hus finally among the saints. A communist festival in commemoration of Hus in

1921 went under the slogan, "Hus and the first Communists in Bohemia."

Hus, who shows up in Macek's work as a warm and generous man, would have been amused.

RICHARD MARIUS  
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Knoxville

RICHARD CLOGG, editor. *The Struggle for Greek Independence: Essays to Mark the 150th Anniversary of the Greek War of Independence*. [Hamden, Conn.:] Archon Books. 1973. Pp. vi, 259. \$13.00.

This volume appeared to mark the one hundred fiftieth anniversary of the outbreak of the Greek war of independence in 1821. It is a collection of well-researched essays that deserve the attention of scholars of modern Greek history and literature.

Richard Clogg introduces the essays in his scholarly paper, "Aspects of the Movement for Greek Independence." He discusses the direct inspiration of the French Revolution on the Greeks of the Diaspora in their efforts to create a Greek state, in contrast with the antipathetic spirits of the notables—the high clergy and the Phanariots—who were wedded to the Ottoman status quo. He also underlines the tensions that existed in Greek society itself and that developed into factionalism and feuds during the course of the eventually successful struggle for Greek independence.

In his essay "The Phanariots and the Byzantine Tradition," Cyril Mango, by passing general judgments, presents the Phanariots as a selfish, politically backward class of Greeks who thought only of increasing their wealth and satisfying their ambitions by remaining subdued to the Ottomans, with the assistance of the Church that they financially and politically controlled. He goes further to prove that the literary culture of the Phanariots was entirely devoid of merit and, in comparison to Byzantine literature, was lacking in any "poetic feeling"—statements that historians may find surprising. Mango raises questions that only the use of the enormous mass of the existing sources, not yet fully exploited, would answer. Already Romanian scholars have shown the positive contribution of the Phanariots to present-day Romania.

Catherine Koumariou, by making use of archival material, evaluates in a realistic manner the significance of the Greek intelligentsia

(including the Phanariot) in the development of the movement for independence (1798–1821), which in turn determined the course modern Greek society was to take.

George Frangos's essay, "The *Philiki Etairia*: A Premature National Coalition," illumines the prerevolutionary national society that led to the outbreak of the struggle but failed to survive it. In this skillful study, the result of extensive research, the author analyzes the social, psychological, and political transformation of the *Etairists*, and, according to the level of development of this transformation, he places the *Etairia* midway between more advanced organizations like the Italian *Carbonari* and the Russian Decembrists.

In his paper "Kapodistrias and the *Philiki Etairia*," C. M. Woodhouse examines, with his usual distinct style, the circumstances of Kapodistrias's complicity in the plot of 1821. The author concludes that until more documents are unearthed to alter it, the truth about Kapodistrias's position lies between tacit acquiescence and explicit approval.

"The 1821 Revolution in the Rumanian Principalities" by E. T. Tappe is a well-wrought essay on the tangled story of the Principalities under the Phanariot princes in 1821. The author, making use of Romanian printed sources and British diplomatic records, analyzes the events that led to the disastrous end of the uprising in the Danube.

With his deep knowledge of the period Douglas Dakin, in his article "The Formation of the Greek State, 1821–1833," gives in a comprehensive and clear manner the story from its social, political, military, and administrative point of view, and he ends up with a discussion of the diplomacy that created the modern Greek state.

Philip Sherrard, in his lucid and penetrating essay "Church, State and the Greek War of Independence," argues that the roots of the Greek Church's separation from the Oecumenical Patriarchate lie in the theory and practice initiated by Constantine the Great. The author examines the tradition of the Eastern Church and how it was influenced by ideas of the conception of Church-state relationships that developed in medieval and postmedieval Western Europe. This conception went down through the intellectuals of the Diaspora and Von Maurer.

In his well-researched essay "The Other British Philhellenes," Alexis Dimaras examines the efforts of the nonmilitary Philhellenes, usually forgotten by the historians, to spread

knowledge among the Greeks by establishing schools, introducing printing presses, or translating from texts such as Bentham's.

Finally, Robin Fletcher, in his study "Byron in Nineteenth-century Greek Literature," analyzes the works of Greek poets and concludes that with difficulty there are to be found traces of Byronic influence. Fletcher's paper is not only an up-to-date survey, but it stands as a commemoration of the one hundred fiftieth anniversary of Byron's death in 1824.

DOMNA VISVIZI-DONTAS  
Archives of the Greek  
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NICOLAE COPOIU. *Le socialisme européen et le mouvement ouvrier et socialiste en Roumanie, 1835–1921*. (Bibliotheca Historica Romaniae, Études, 45.) Bucharest: Editura Academiei Republicii Socialiste România. 1973. Pp. 207. Lei 8.25.

DAN BERINDEI. *L'année révolutionnaire 1821 dans les pays roumains*. (Bibliotheca Historica Romaniae, Études, 46.) Bucharest: Éditions de l'Académie de la République Socialiste de Roumanie. 1973. Pp. 245. Lei 10.

The Bibliotheca Historica Romaniae series of historical studies is designed to acquaint the non-Romanian reader with the work of contemporary Romanian historians and with the principal problems of contemporary Romanian historiography. The volumes by Nicolae Copoiu and Dan Berindei fulfill that purpose.

The common denominator of the two studies is the authors' attempt to present their respective topics in the context of Romanian history as such and as *pièces justificatives* for the historic legitimacy of the Romanian communist regime and of the Socialist Republic of Romania. Officially that legitimacy is based on the Romanian Communist party's realization of the main strands of the Romanian historical legacy—the social and national revolutionary traditions of the Romanians.

Copoiu's study on socialist and workers' movements is somewhat less persuasive in that respect than Berindei's on the revolutionary year 1821. This is in part because of the weakness of the workers' and socialist movements in Romania between 1835 and 1921, which precludes any analysis in depth or, for that matter, much more than superficial extrapolation of their significance for contemporary history. The author makes a valiant effort in tracing the history of the impact of European socialist ideology on Romania in the nineteenth and

early twentieth centuries but succeeds only in persuading the reader that the impact was limited by the retardation of Romania's industrial development and by the generally low political consciousness of the Romanian workers and of their immediate leaders. Copoiu's account of the development of the workers' movement in Romania is more valuable since it concerns itself with an actual movement—albeit a modest one—and is based on the best primary sources available. The deliberate attempt to show resemblances between the *modus operandi* of the international and national socialist movements of the period under discussion and of those of today detracts from the value of the book. The implied parallelism between socialist internationalism of the late nineteenth and late twentieth centuries is somewhat tenuous, and the repeated reassertions of the unity of socialist purpose during the crucial years antedating World War I, the Bolshevik Revolution, and the establishment of the Romanian Communist party are formalistic rather than realistic.

Berindei's excellent study on 1821 is less affected by problems of interpretation arising from adherence to the goals of the editors of the *Bibliotheca Historica Romaniae*. He provides a learned and incisive analysis of the antecedents to the revolution of 1821 and of the revolution itself, stressing, correctly, the social, antifeudal character of the movement. Nevertheless, Berindei is not immune from contagion with the prevalent theory that the revolutionary movement of 1821 was as much national as it was social in character, that in fact the leader of the movement, Tudor Vladimirescu, sought to represent the entire Romanian nation in his struggle against Greek and boyar. Stated in these terms it is indeed possible to view the revolution of 1821 as a national movement, although it is probably more accurate to regard it as an anti-Greek manifestation, and as such national only in a narrow sense of the word, than as a conscious political manifestation for and by the Romanian nation. The accuracy of the corollary contention that the revolution of 1821 was the immediate antecedent of the revolutions of 1848 and, by extension, of all subsequent national and social revolutions that culminated in the creation of the Socialist Republic of Romania cannot be challenged in the context of Berindei's analysis.

Both Copoiu's and Berindei's studies are important contributions to the history of Romania and to the understanding of con-

temporary Romanian historiography and its problems.

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WILLIAM O. OLDSON. *The Historical and Nationalistic Thought of Nicolae Iorga*. (East European Monographs, 5.) Boulder, Colo.: East European Quarterly; distrib. by Columbia University Press. New York. 1973. Pp. 135. \$10.00.

The sheer weight of Iorga's writings (about 1,200 books and pamphlets, 13,000 articles, 5,000 book reviews, and a daily newspaper) probably discouraged any Westerner from plowing through this incredible mass to secure an insight into this Romanian historian's contributions. Now there comes along a slim, 135-page analysis of Iorga's thought by a young American scholar at Florida State University who undertook research in Romania. Whereas Iorga was accorded a few sentences in standard studies of historiography (Gooch, Gardiner, Fitzsimmons, and others), we now have an analysis of the distinctions between Iorga's history and propaganda, although he probably could not distinguish between them. Oldson clearly shows that Iorga (1871–1940) projected his own ideas on what nationalism should be for the purpose of fulfilling the needs, as he saw them, of the Romanians. To Oldson Iorga was eclectic, "seeking everywhere for support of his contention that the Rumanians must be unified and must possess all the territory of post-World War I Rumania." Iorga's fervent nationalism, discredited by Romanian Marxists until about 1964, has now ironically added a considerable substructure to current Romanian historiography. A critic once told Winston Churchill, one of England's most vocal nationalist historians, that his account of a medieval battle was not accurate. The great imperialist retorted, "I know. But that is the way it should have happened!" That gem might very well apply to Iorga, a contemporary of Churchill, in his reordering of historical evolution. A revival of interest in his thought should produce a demand for more of Iorga's works in English; the only item of any consequence is a one-volume history of his country published in 1925! None of the sources attributed to Iorga in Oldson's study is in English. Until more of Iorga is available in translation, reliance must be placed upon analyses such as this seminal study.

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V. CURTICĂPEANU. *Le mouvement culturel pour le parachèvement de l'État National Roumain* (1918). (Bibliotheca Historica Romaniae, Monographies, 12.) Bucharest: Éditions de l'Académie de la République Socialiste de Roumanie. 1973. Pp. 264. Lei 23.50.

In this book on the Romanian national movement in Transylvania and among Transylvanians elsewhere the author shows how a modern form of national consciousness, stressing the abstract notions of equality before the law, liberty, fatherland, and nation, undermined the traditional emphasis on hereditary privilege, personal dependence, feudal territorial organization, and rank and social condition. One of the main ways of achieving this was through the formation of societies aiming to foster a uniform national language and literature, a Latin orthography, a Romanian historical consciousness, and a Romanian national economy. The territorial basis of these associations was variable. Each territorial type, however, strove to replace the separatism of older forms of territorial organization (village, manor, city, province) with a sense of personal identity between the speakers of any Romanian idiom and all the territories on which Romanian idioms were widely spoken.

The author devotes a chapter each to post-1848 local cultural associations with a national orientation; to regional cultural associations—the Maramureș society with headquarters in Sighet and a focus on northwestern Transylvania, the Banat society with headquarters in Arad, the Astra society with headquarters in Sibiu and an initial focus on “historical” Transylvania—that were virtually all founded in 1861 or 1862; and to the later extension of Astra activity to all of Transylvania. Astra's membership grew from 745 persons in 1862 to 1,023 in 1896, 1,980 in 1909, and 10,300 in 1914. In 1907 it undertook the publication of a periodical for peasants (*Tara noastră*). In 1912 it transformed the periodical *Transilvania*, founded in 1868, from a relatively learned review into a propaganda vehicle.

Other chapters concern student associations in Vienna and Budapest, Austro-Hungarian censorship and the diffusion of Romanian books to Transylvania, and the role of the Romanian monarchy and of Bucharest and Jassy in furthering relationships with Transylvania through the Romanian Academy (founded in 1866 and given this name in 1867), through Transylvanian emigrants, especially intellectuals, who found employment in Romania, and through the formation in 1908 by the historian Nicolae

Iorga of a prestigious people's summer university in Văleni-de-Munte, near the Transylvania frontier. While lacking the right to grant diplomas and paying no salaries, Iorga's university was able to attract both famous teachers and eager students, especially from Transylvania, where the Romanians had failed to obtain authorization to establish a university.

In important albeit unspectacular ways the cultural associations of the Transylvanians and other Romanians helped to undermine the ideologies of dualism and of Austro-Marxist reformism, which intended to deprive the nationalities of Austria-Hungary of a territorial basis. More affirmatively, they aided the Romanian peasant “to escape his patriarchal isolation, learn to practice a rational agriculture, turn toward commerce and the crafts, and make contact with modern life by broadening his political and cultural horizon” (p. 250).

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ILIJA JUKIĆ. *The Fall of Yugoslavia*. Translated by DORIAN COOKE. New York: Harcourt Brace Jovanovich. 1974. Pp. 315. \$8.50.

This personal but readable study is a concise treatment of the complex internal and external development of Yugoslavia from the period immediately preceding the Second World War to the resounding proclamation of the Yugoslav republic on November 29, 1945. The author served his country in the ministry of foreign affairs, first in Belgrade and later in exile in London. In these capacities Jukić was able to observe the origin and evolution of the Yugoslav tragedy and thus produce the present work.

The frightful conditions in Yugoslavia during the Second World War were foreshadowed by the nineteenth-century *Kremna* prophecy that foretold such a dreadful situation that the living would “envy the dead.” Although the gap between prophecy and reality can often be large, such was not the case in Yugoslavia. The ghastly state of affairs there, described by Winston Churchill as “a tragedy within a tragedy,” can be assessed from official Yugoslav statistics: 1,706,000 dead, 425,000 disabled, 1,610,000 imprisoned, and 525,000 children orphaned.

The vital problems confronting the Yugoslav state during the occupation included the Croatian-Serbian hostility, the Mihajlović-Tito con-



flict, tense relations between the Yugoslav and occupying forces, and the lack of communication between the government in exile and the military leaders in Yugoslavia itself.

The foregoing factors made the situation in occupied Yugoslavia most complex. Jukić notes that increasing preoccupation of Yugoslav politicians in exile with personal self-interests compounded the problem. Revelation of apparent Croatian and Serbian chauvinism, together with the determination of some officials to capitalize on contentious issues, divided the cabinet. To Jukić, who witnessed them, such developments increased skepticism and division among the Yugoslavs. As an intermediary, the author himself believed in maintaining flexibility and restraining the various political factions. This goal proved unattainable in the heated atmosphere of the period. The Yugoslav cabinet, unable to distinguish between peripheral and vital interests, missed the opportunity to create a noncommunist resistance that might have successfully challenged partisan leadership. Jukić states that Tito's skillful maneuvering and deceptive behavior persuaded the Yugoslav people and the Allies to do what was necessary to ensure the safety of the country. The author brands England as overoptimistic and unable to assess the effects of its actions. Still, Jukić somberly concludes that in spite of Allied misjudgments, the successive Yugoslav governments bear a large responsibility for the rise of Tito's dictatorship.

This volume includes a bibliography and an index, but it is regrettable that it lacks footnotes. While Jukić's personal and somewhat pretentious judgments are unavoidable and must not be overlooked, his microscopic but purposeful study gives ample matter for reflection for both specialist and student. The work adds considerably to our accumulation of details on the subject.

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EDWARD L. KEENAN. *The Kurbskii-Groznyi Apocrypha: The Seventeenth-Century Genesis of the "Correspondence" Attributed to Prince A. M. Kurbskii and Tsar Ivan IV.* With an appendix by DANIEL C. WAUGH. (Russian Research Center Studies, 66.) Cambridge, Mass.: Harvard University Press, 1971. Pp. xi, 241. \$10.00.

Professor Edward Keenan's research work is sensational. While the Harvard University Press considers it an outstanding scientific dis-

covery, Soviet historians and Slavists, however, tend to agree with the critical appreciation of it by Nikolay Andreyev (Cambridge University), Marc Szeftel (Washington University), and Sergei Zenkovsky (Vanderbilt University). Their reasons for this are that a special study of the Groznyi-Kurbski "correspondence," based on criticism of sources, had already begun at the end of the nineteenth century, and such great Russian scientists as S. F. Platonov and I. I. Zhdanov had taken part in it. In 1914 G. Z. Kuntsevich published, on the initiative of Platonov, the first volume of *Sochinenia Kniazia Kurbskogo* (proofs of the second volume, which was not released on account of the war, are kept in the archives of the Leningrad branch of the Institute of the History of the USSR under the Academy of Sciences of the USSR). Later an academic edition of *Poslaniia Ivana Groznogo* (1951), prepared by D. S. Likhachev and Ya. S. Lurie, was published together with an extensive scientific commentary. From the 1950s through the 1970s the problem of the literary heritage of the dreaded tsar and the disgraced boyar became the subject of special research, based on criticism of sources; not only the famous "epistolae" but also the well-known additions to the *Lizvye svody* chronicle of the mid-sixteenth century (sometimes attributed to the tsar) as well as Kurbski's *Istoriia o velikom kniaze Moskovskom* came under investigation. We have in view researches by Soviet scientists—D. N. Alshits, D. S. Likhachev, A. A. Zimin, S. O. Schmidt, R. G. Skrynnikov, K. A. Uvarov, M. V. Lyapon, and Yu. D. Rykov—as well as the works of our colleagues in England, Ijorn Fennell and N. Ye. Andreyev. However, none of these representatives of the "old" and "new" schools of criticism of sources ever doubted the reality of the Ivan Groznyi-Kurbski "correspondence" and consequently never declared (as Keenan did) that both their letters and Kurbski's *History* were mere literary falsifications perpetrated in the seventeenth century. At the same time, Keenan even names the first author of this literary "jest," who "he discovered" was the writer, Prince S. I. Shakhovskoi. This discovery is, as we see, equally bold and original, but the author (and this is the main point) does not corroborate it by serious scientific argumentation.

The basic proof of Keenan's "discovery"—the key to the solution of his theorems related to criticism of sources—is, in Keenan's opinion, the collation of Kurbski's first letter to Ivan Groznyi (1564) with the so-called *Complaint* by



the West Russian monk Isaiah that was written, in Keenan's opinion, in 1566. Having noticed the similarity of certain canonic examples and expressions in the letter and the *Complaint*, Keenan considers this fact sufficient for asserting that Kurbski's first letter to Ivan Groznyi could not have been written, as is shown by the contents of the letter, in 1564, that is, immediately after his defection to Poland. According to Keenan this letter must have been composed only after Isaiah's *Complaint*, and therefore its author is not Kurbski but a person who used Isaiah's message while working on the texts of the false "correspondence." Only this assumption makes credible Keenan's reasonings based on criticism of sources and his system of logical theorems—assumptions that follow. Let us note, moreover, that in Keenan's book specific historical analysis of sources is of secondary importance, as compared with the formal-logical process of comparing them to each other. Therefore, if we do not accept the first "theorem," this will cause the fall of the whole of the author's hypothesis. The above has been proven by noted Soviet scientists—D. S. Likhachev and R. G. Skrynnikov in their thorough reviews of Keenan's work (Likhachev's "Kurbskii i Groznyi—Byli li oni pisateliami?" in *Russkaia literatura* [1972] and Skrynnikov's *Perepiska Groznogo i Kurbskogo. Paradoxy Eduarda Keenana* [Leningrad: Izdatel'stvo "Nauka"; 1973]). Arguments of both authors are mutually complementary. Likhachev is concerned mostly with the literary and linguistic analysis of the "correspondence," and Skrynnikov the historical one. Thus Skrynnikov clearly demonstrates how very tendentious and contradictory Keenan's "argumentation based on criticism of sources" is. To a large extent this is because of the fact that the author did not work on the manuscripts in person (they have been reviewed, textologically characterized, and dated by Daniel C. Waugh). Referring to Keenan's principal "theorem"—the correlation of Kurbski's first letter (1564) with Isaiah's *Complaint*—Skrynnikov comes to the conclusion that the latter was written not in 1566 (the *Complaint* bears no date), but in 1562, and therefore the correlation of the texts is the inverse of Keenan's: Kurbski himself used in 1564 the text of Isaiah's *Complaint* written in 1562. Likhachev gives a different interpretation of the "fact" of the above-mentioned borrowing of texts: he believes that both Kurbski and Isaiah (whether the latter wrote his *Complaint* in 1562 or 1566)

made use of the same texts of church origin or literary commonplaces—a phenomenon very typical of both Russian and West European sixteenth-century literature.

To this should be added that all of Keenan's assumptions—his story of the allegedly false Groznyi-Kurbski "correspondence"—are very artificial in both aspects—that of the criticism of sources and of general history. That story consists much more of questions and assumptions than of concise analysis. It remains in blatant contradiction with all we know about the reign of Ivan the Terrible, and the more so about the history of Russian culture in the sixteenth century. Keenan sometimes exhibits a most strikingly nihilistic attitude to special scientific literature on sixteenth through seventeenth century Russian history. As a matter of fact, he does not even ask himself how it happened that the authors of the "correspondence" and Kurbski's *History* (if both are just seventeenth-century forgeries) were so strikingly well informed on events of the mid-sixteenth century. At the same time practically all data on Ivan the Terrible's childhood, the Selected Council, the terror of the "Oprichnina," and so on, which are found in the "correspondence," are corroborated by other data contained in sixteenth-century sources now at our disposal; the majority of these sources, both private and official, could not be known either to Prince S. I. Shakhovskoi or his literary colleagues. As to Kurbski's *History*, the most reliable proof of its veracity is contained in the tsar's Synodics (list of persons killed during the "Oprichnina"), which were compiled from the 1670s through the 1680s. An interpretation of the very restricted data of the Synodics is possible only on the basis of information supplied by Kurbski. Therefore Keenan's work clearly does not comply with the real sixteenth-century history of Russia.

We believe that the present discussion on the Groznyi-Kurbski "correspondence" may yield most fruitful results, not along the lines set in works of the type of Keenan's, but on the basis of an objective, comprehensively critical study of all sources, in their entirety, now at our disposal on the history of Russia in the sixteenth century. This analysis must not be subjected to any schemes that are planned in advance or to the quest for new scientific sensations.

As one of the first steps in this direction we recommend the preparation of a complete scientific edition of the whole epistolary and pub-

licistic heritage of Groznyi and Kurbski. The need for such a publication has been frequently pointed to both in Soviet and foreign historiography.

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GEORGE V. LANTZEFF and RICHARD A. PIERCE. *Eastward to Empire: Exploration and Conquest on the Russian Open Frontier, to 1750*. Montreal: McGill-Queen's University Press. 1973. Pp. x, 276. \$15.50.

This work describes the exploration and conquest of Siberia by remarkably small bands of Russian adventurers, some in governmental service, some as private entrepreneurs. The principal author, Richard Pierce, states that the book is a survey and not a monograph. Indeed, the book is not the result of archival research and is based largely on secondary sources, but these sources and a good deal of published primary data are well used. Overall the book, with fifteen maps, is a welcome contribution and should prove to be particularly valuable to nonspecialists.

The major shortcoming of *Eastward to Empire* arises from the fact that the first six chapters were put together from the notes of the late George Lantzeff. Pierce was perhaps too respectful of those notes, for the first four chapters are quite dull and not very relevant to the overall purpose of the book. In these chapters Russia's eastward expansion during the Kievan and Mongol period is discussed in a loose fashion that reads like a rough draft for a general survey of Russian history from the tenth to the mid-sixteenth century. The opening chapters may well discourage readers from proceeding further, which would be an unfortunate mistake. For the remainder of the book is quite good, although at times one tends to get lost in a forest of unfamiliar personal and geographical names.

In his concluding chapter Pierce discusses some of the causes and consequences of Russia's eastward expansion. It is unfortunate that he does not emphasize one important point: that the social structure of European Russia precluded significant emigration to the east; that for this reason the conquest of Siberia in the seventeenth and eighteenth centuries did not in any important way alter the main line of Russian historical development of that period.

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N. E. NOSOV *et al.*, editors. *Problemy krest'ianskogo zemlevladieniia i vnutrennei politiki Rossii: Dooktiabr'skii period* [Problems of Peasant Land Ownership and the Internal Policy of Russia: Before the October Revolution]. (Akademiia Nauk SSSR, Institut Istorii SSSR, Leningradskoe Otdelenie.) Leningrad: Izdatel'stvo "Nauka." 1972. Pp. 363.

The Leningrad section of the Institute of History in the Soviet Academy of Sciences has over the years published a series of significant contributions to the study of Russian history. A considerable part of its research has been devoted, with due consideration to the current political climate, to Russian history in the "feudal" era and to the tsarist regime. The present volume continues this orientation with perhaps a freer range of subject matter and treatment. Ideology has faded away; the bias now seems to be a common resentment of "feudalism" thwarting or destroying sound beginnings in Russian historical evolution.

The contents of the present volume come in two parts. The lesser part contains four essays (D. I. Raskin, I. Ia. Froianov, and A. L. Shapiro on "The Forms of 'Black' Peasant Farming in the 14th to the 17th Century"; N. E. Nosov on "Two Tendencies in the Development of Feudal Agriculture in Northeast Rus' in the 15th and 16th Century"; Iu. G. Alekseev on "The Peasant Volost in the Center of Feudal Rus' in the 15th Century"; and A. I. Kopanov on "Peasant Agriculture in the Dvina Region in the 16th Century"), all of which show the vigor of peasant life, including even the beginning of capitalist enterprise among them.

The second and more substantial part deals with official and nonrevolutionary Russia between the emancipation and the October Revolution, probing into several directions where clarification is highly welcome. V. G. Chernukha's chapter, "Problems of Political Reform among the Ruling Circles of Russia at the Beginning of the 1870's," deals with the political views of Prince Shuvalov, an ardent Anglophile, and the creation of the Valuev Commission. T. M. Kapitana, "The Economic Incorporation of the North and the Railway Tariff Policy of S. Iu. Witte," shows how the Cheliabinsk base point diverting Siberian grains to northern ports, in effect from 1896 to 1911, fitted into Witte's policy of developing the northern region, including Murmansk, and at the same time protected the "feudal" landowners in central Russia. Iu. B. Solov'ev reports on the controversy between the liberal Boris Chicherin and several conservative spokesmen over the

role of the landowning nobility at a time of considerable public debate over the future of Russia. V. C. Diakin carries the same theme to the opposition of the landowning nobility to Stolypin's efforts to revamp rural administration. L. E. Shepelev next provides a detailed and useful survey of tsarist legislation dealing with joint-stock companies and of the continuing fear among the ruling circles of Jewish enterprise. The last two articles carry the analysis of "reactionary" elements past the February revolution. V. H. Ginev shows how leading theorists among the Soviet Revolutionaries leaned increasingly toward the kulaks. P. Sh. Ganelin, finally, undertakes a close scrutiny of the forces and considerations surrounding the long-debated proclamation of the republic in 1917.

All contributions to this volume are informative and useful. They deserve the respect and attention of American scholars of Russian history, who will find them hardly different from their own best efforts.

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N. A. KRAVCHUK. *Massovoe krest'ianskoe dvizhenie v Rossii nakanune Oktiabria (Mart-oktiabr' 1917 g., po materialam velikorusskikh gubernii Evropeiskoi Rossii)* [The Massive Peasant Movement in Russia on the Eve of the October Revolution (March–October 1917, on the Basis of Materials from the Great Russian Gubernias in European Russia)]. Moscow: Izdatel'stvo "Mysl'." 1971. Pp. 276.

A. M. CHINCHIKOV. *Sovetskaia istoriografiia sotsialisticheskogo preobrazovaniia sel'skogo khoziaistva SSSR (1917–1969 gg.)* [Soviet Historiography of the Socialist Reform of the Rural Economy of the USSR (1917–1969)]. (Akademiia obshchestvennykh nauk pri TsK KPSS, Kafedra istorii SSSR.) Moscow: Izdatel'stvo "Mysl'." 1971. Pp. 220.

The Soviet Union, the first self-styled proletarian state, has always been embarrassed by its peasants. Although Russian Communist leaders have had to wrestle with the problems of peasant society, until recently Soviet historians have not devoted much attention to these efforts or to twentieth-century agricultural history in general. In the past few years, now that peasants no longer constitute the majority of the population, more historians have turned to the study of the two great agricultural revolutions in twentieth-century Russia, the peasant seizures of land in 1917 and the collectivization program in which the government conquered

its own peasantry. Kravchuk examines the first of these revolutions in Great Russia on the basis of his own extensive archival research, and Chinchikov surveys Soviet scholarship on the second. Like the work of most Soviet scholars, theirs suffers severely from the need to tailor historical evidence to ideological presuppositions.

In his introduction Kravchuk attacks American scholars (without identifying any of them by name) for assuming that the peasant revolution of 1917 had no relation to the working-class revolution and that the tide of peasant revolt was ebbing in 1917, so that the Provisional Government probably could have solved the problem of land distribution. On the contrary, says Kravchuk, the peasant revolution and the working-class revolution mounted in intensity, converged in October, and realized their common goals under Bolshevik leadership. He follows Lenin in treating the poor and landless peasant as a rural analogue to the proletariat. The two groups must, therefore, have been allied in the struggle that brought success to the Bolsheviks.

Kravchuk is fairly successful in demonstrating on the basis of new data that the number, scale, and violence of peasant disturbances, after dipping in August, rose to new heights in September and October. But the evidence adduced to prove the convergence of interests between worker and poor peasant—attacks on rich farmers, the split in the Socialist Revolutionary party, the adoption by peasant congresses of resolutions for land, peace, and Soviet power, the results of the elections to the Constituent Assembly—does not convince. These developments are subject to many other interpretations than the one Kravchuk supplies, as is evident in Oliver Radkey's well-known studies in English of the Socialist Revolutionary party.

Chinchikov's book is both a survey of Soviet historiography on the development of Russian agriculture since the Revolution and a guide to current orthodoxy on the question. It is difficult to decide which of these goals was most important to the author in writing the book. It would, of course, be too much to ask Chinchikov to question the decision to collectivize agriculture by examining the high cost in human lives and suffering, not to mention agricultural productivity. And despite de-Stalinization, it is apparently not yet possible for Soviet scholars to question Stalin's decisions on the pace and form of collectivization. Within these strictures the author faithfully catalogs

Russian scholarship on Soviet agriculture, even devoting some space to Trotsky, Bukharin, and some of the major non-Bolshevik Russian economists. He also skillfully sorts out the differences between them and identifies some areas of investigation that remain to be explored. The book will be useful to Western scholars for those purposes, though they will be somewhat repelled by Chinchikov's persistent affirmation of the infallibility of the Central Committee of the Russian Communist party. Some of the author's assertions will seem outrageous to Western scholars, such as the statement that "studies show that by 1937 an absolute majority of former kulaks worked in cooperative-collective farms or state enterprises, having had their political rights fully restored."

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IVAN H. EBERSTEIN. *Iantarnyi kraj: Tragediia goroda Libavy i gibel' Rossiiskoi Imperii* [The Amber Land: Libava's Tragic Fate and the Fall of the Russian Empire]. New York: [the author]. 1970. Pp. 327.

Ivan Eberstein's life has been rich and varied: childhood in the Russian empire's Baltic province of Courland as the son of a well-to-do restaurateur; Gymnasium years spent with his family in the Latvian "colony" in a St. Petersburg (Petrograd) that was experiencing the shattering blows of World War I, the revolutions of 1917, and civil war; university studies in independent Latvia and the career of an economist; a year as economic planner for a district executive committee in Soviet Riga; work as a journalist for the German-language press in Nazi-occupied Latvia and on the Caucasus front; migration, after the collapse of the German armies, to Germany and then to Denmark; and emigration to Argentina and, five years later, to the United States, where he taught the Russian language at Syracuse University until retirement to his home on Long Island. Here is material for several books, but Eberstein does not stop there. His book is both a personal memoir and an attempt at historical interpretation on a panoramic scale embracing events and movements from ancient times to the present century. In my opinion the author has tried to do too much; less would almost certainly have been better; the book would probably have suffered less from discursiveness, repetition, and annoying, though generally peripheral, errors of fact.

The focal point for both memoir and panorama is Eberstein's native and beloved city of Libava (today Liepaja in Soviet Latvia) and the neighboring Baltic amber-producing areas—Amber Land. According to the author the ancient Greeks and Romans traveled to Amber Land with some frequency, lured by the glowing golden brown gem that was highly valued for its beauty and its supposed magical properties. Desire for the jewel reached an extreme point in the first century A.D. when a kind of "amber fever" swept through Nero's Rome. After the Roman Empire's fall other peoples and cultures continued the trek to the Baltic. Eberstein sees many developments in medieval and modern history linked to the quest for amber, and he believes the Baltic area came to bear a special imprint because of this.

When Peter the Great fell in love with the Baltic Sea and Western technology and built his "window to west" on the very edge of Amber Land, and later when various eastern Baltic territories, including the duchy of Courland, became part of the Russian empire, the cultural imprint and cosmopolitan outlook of Amber Land were transferred to the new tsarist capital. St. Petersburg, thus, was the final link in a historical chain going back to Greece and Rome. The beginning of the end for Petersburgian Russia came in 1894 when Nicholas II, overriding the preference of his most important minister, Witte, and of certain of the Romanovs, as well as the expressed wish of his recently deceased father, cancelled plans for establishing a new naval base at Murmansk and located it, instead, at Libava. Results flowing from this, Eberstein believes, included intensification of intrigue at the tsarist court and a complex shift in relations with Germany, both of which contributed to the outbreak of war, Russian defeat, and a revolution that destroyed the Russia of St. Petersburg.

Parts of this vast interpretive schema are supported by citations to Baltic legends, etymology, Greek mythology, Roman historians, Icelandic sagas, archeological findings, the writings of Arnold Toynbee, and other sources. Parts, the author frankly states, are unsupported hypotheses for testing and elaboration by future historians.

The most interesting and valuable materials in the book for me were Eberstein's descriptions of Libava during the first decade and a half of the present century, when the city was experiencing a golden age of commercial expansion and importance as the embarkation point for Russian Jewish emigration to the United

States, and his account of life as a student in war-torn, revolutionary Petrograd.

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ANTHONY C. SUTTON. *Western Technology and Soviet Economic Development, 1945 to 1965*. Stanford: Hoover Institution Press, Stanford University. 1973. Pp. xxxi, 482. \$15.00.

With this volume Mr. Sutton completes a three-volume study of the role of Western technology in Soviet economic development from 1917 to 1965. The study as a whole is an important contribution to our understanding of the industrialization process in the USSR. The wealth of technical detail painstakingly culled from a wide variety of source materials will provide valuable inputs into future studies of the development of individual industries and analyses of the mechanics of technology transfers among nations. Judging from the study as a whole, the role of Western technology in fueling Soviet industrial growth has been underestimated in the past. Mr. Sutton is to be commended for undertaking the laborious task of assembling the evidence.

In many respects the final volume of the study is much less convincing than the earlier ones. Seventeen chapters, nearly half of the volume, detail industry-by-industry the Western origins of nearly all plant and equipment acquired either through direct imports, duplication of imported designs and equipment, or lend-lease and reparations. Although the description of acquisitions from the latter sources is of value, the ultimate economic benefits probably were considerably less than might be assumed from listings of plants dismantled and equipment transferred. The description of the technology acquired through trade in the 1950s and 1960s seems superficial. The mass of technical detail presented for the various industries urgently needs to be integrated with available data on trade, investment, and output.

The publication of the final volume, covering the postwar period, is particularly timely, coinciding as it does with a new drive by the USSR to obtain another massive infusion of Western machinery, equipment, and technical know-how to modernize its economy and to aid in exploiting sources of raw materials. It is clear that the two previous major infusions—through technical aid agreements with Western firms to build and equip many large industrial plants in the 1930s and through the transfer of large

amounts of United States and European technology by lend-lease and postwar reparations in the 1940s—have greatly facilitated Soviet industrial development. Soviet state ownership and central planning have proved notably unsuccessful in generating indigenous innovation and rapid diffusion of technology. While perhaps agreeing with Sutton's concluding statement that "Soviet central planning is the Soviet Achilles' heel," one may nonetheless quarrel with his broader assertion that "all along the survival of the Soviet Union has been in the hands of Western governments." In Sutton's view, Western trade with the Soviet Union amounts to aiding the enemy.

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### NEAR EAST

KEMAL KARPAT. *An Inquiry into the Social Foundations of Nationalism in the Ottoman State: From Social Estates to Classes, from Millets to Nations*. (Research Monograph number 39.) Princeton: Center of International Studies, Woodrow Wilson School of Public and International Affairs, Princeton University. 1973. Pp. 116. \$4.00.

The purpose of Professor Karpap's monograph seems to be twofold: to debunk certain "myths" about the inner workings of the Ottoman empire and, more importantly, to offer a new socioeconomic explanation for the shifts and changes that led eventually to the rise of national movements. His study covers the Muslim and non-Muslim subjects in the Balkans and Middle East as a whole and includes yet another set of charts to illustrate his conception of the divisions of society before and after the eighteenth century.

Karpap attributes Ottoman success for almost four hundred years in governing a heterogeneous population to what he proposes was a new administrative system based on "social estates" reflecting occupational differences that united the subjects by cutting across ethnic and religious boundaries, and on millets, in regard to which the government adopted a positive attitude of noninterference and nonintervention. In his view, it was the disintegration and restructuring of this system that led ultimately to the nationality problem.

Others, of course, have shown that the administrative and governmental ranks were not exclusive as to religious or ethnic affiliation. Schematically, moreover, the picture posited by



Karpat does not, in the end, seem too different from the traditional view of a vertical division of society between rulers and ruled and a horizontal organization of the ruled into millets, including a Muslim one. What is new is his emphasis on the socioeconomic factor behind the transformation of the social estates into "primitive forms of social classes" (p. ii) and the millets into nations. It is the material presented to bolster this interpretation that provides both the most fruitful and interesting parts of this study and its more debatable points. Karpat prefers to see the rise of nationality and nationalism as an "ideological justification for a specific form of socioeconomic and political reorganization" (p. iii), and he insists that the nationality problem was a recent one, which appeared only in the eighteenth century largely as the consequence of social and economic change (p. 7).

The descriptive and narrative parts of this monograph are often better and more consistent than the analytical; one also has to make allowance for hyperbole and some rather outlandish statements. Was the chief reason for the downfall of the Ottoman empire its inability to identify fully with a social class (p. 115)? Can one seriously talk about the "occupational capacity of the basic types of human character and temperament" (p. 19)? Nevertheless, Karpat has assembled much valuable and provocative material to support his theses, has introduced some generally inaccessible sources, and has been careful to define his terms. He also quite rightly calls attention to the need for further demographic studies. His contentions will most likely set off the debates he foresees; if these help to elucidate Ottoman history without further muddying the waters, then his efforts will have been worthwhile.

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KEMAL H. KARPAT *et al.* *Social Change and Politics in Turkey: A Structural-Historical Analysis.* (Social, Economic and Political Studies of the Middle East, volume 7.) Leiden: E. J. Brill. 1973. Pp. ix, 373. 88 gls.

This volume is dedicated to the fiftieth anniversary of the Turkish Republic 1923-73, and it consists of nine essays. Six of these studies are contributions by specialists on Turkish affairs and three are by the editor, Kemal Karpat of the University of Wisconsin. These studies, which were originally presented at a conference

sponsored by the Social Science Research Council and New York University in 1965, have been updated and organized into three sections: historical and structural background, major social groups, and ideology and political action since the 1960 revolution.

The first chapter by the editor sets the stage. D. A. Rustow (City University of New York) traces modernization from Ottoman times (ch. 2). The emerging middle class is described by Nezih Neyzi, director of a Turkish market research organization (ch. 3). Bulent Ecevit, prime minister during 1973-74 and the first Turkish minister of labor (1961-65), presents an incisive discussion on Turkish labor as a new social and political force (ch. 4). John Kolars (University of Michigan) has an interesting study on the integration of the village into the national life of modern Turkey (ch. 5), while Ilhan Basgoz (Indiana University) writes about the "free boarding schools" of Turkey (ch. 6). A key chapter by the editor analyzes the emergence of social groups and expounds on the evolution of the political system since 1960 (ch. 7). Also included is an essay on provincial party politics by Frank Tachau (Illinois State University, ch. 8). The final chapter by Karpat is a concluding synthesis of Turkish ideology since the 1960 revolution.

I agree with the theses in this book, that is, that the Turks are not doctrinaire but pragmatic; that extremism of the right and left are not popular and that moderate policies will prevail; and that the Turkish military have played a positive role supporting democratic institutions. The first fifty years of the Turkish Republic have seen a traditional society, based largely on an agricultural economy, gradually develop into an increasingly industrial country. Despite the growing pains of a society in transition, democratic institutions appear well established.

Subsequent to publication of this volume, important events have occurred. However, the basic conclusions of this study remain valid.

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GEORGE M. HADDAD. *Revolutions and Military Rule in the Middle East. Volume 3, The Arab States. Part 2, Egypt, the Sudan, Yemen and Libya.* New York: Robert Speller and Sons. 1973. Pp. 444. \$12.50.

This is the final book in a three-volume series on the history of military rule in the Middle East. It is written in a lucid narrative style

and is a rather comprehensive survey of the planning, execution, and results of the military coups d'état and revolutions occurring in Egypt, the Sudan, Yemen, and Libya from 1948 to 1969. The military regimes in each of these countries are discussed separately and then compared with one another, and general patterns of military rule in the Arab states are formulated. Although the author attempts to review the causes, execution, and results of each coup d'état in detail, individual coups and military regimes do not receive in-depth analysis because of the magnitude of the subject. The author's generalizations about and comparisons of Arab military coups d'état, however, seem valid and useful in pointing out the trends of unrest, instability, and power struggle in the contemporary Arab world. Still, to describe the political situation in Arab countries in all its complexity is an ambitious attempt on the part of the author.

Haddad places primary emphasis on the inadequacies and failures of the Arab military regimes, giving special attention to Nasser and the effects of Nasserism in Egypt and in other Arab countries, especially Syria and Yemen. He is critical of the Arab military regimes for abolishing political parties; denying Arab citizens basic civil liberties; refusing to allow popularly elected assemblies; making inadequate social reforms; suffering disastrous military defeats in their halfhearted attempt to liberate Palestine; creating an artificial class struggle that was really a projection of their own attempt to break the power and prestige of the middle class; attempting to spread socialism throughout the Arab world; and aligning themselves with communist governments and turning against the Western powers. The author, however, takes a one-sided view of this shift in alignment, pointing out the dangers of Soviet influence while remaining mute on the implications of strong intervention by the West. Military leaders cited United States and Western support of Israel and a desire for independence from imperialistic rule as reasons for turning away from the West. Rather than accepting these as valid reasons, the author feels that they were generated primarily by Soviet propaganda.

Egypt's disastrous military defeat in 1967, the author assumes, is an irrefutable indication of the corruption and weakness of Arab military rule. This conclusion might be reconsidered in light of the military confrontation of October 1973.

Rather too easily the author offers one "logi-

cal" solution to the complex political turmoils of the Arab world. He asserts that army officers should go back to their barracks, and he recommends that a liberal democratic system of government, run by elected officials, replace military rule. This suggestion permeates each chapter of the book, especially the concluding one in which he offers generalizations about the outcome of all Arab military regimes. However, the author makes few suggestions as to how the Arabs can prepare themselves for an effective democratic system of government that would not lead to anarchy. Neither does he make any specific suggestions as to how this democratic system should evolve.

Although the author has gathered abundant information in this work, his heavy reliance on ministry of information publications and newspaper articles raises the question of reliability.

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SAMUEL J. ROBERTS. *Survival or Hegemony? The Foundations of Israeli Foreign Policy*. (Studies in International Affairs, number 20. The Washington Center of Foreign Policy Research, School of Advanced International Studies, Johns Hopkins University.) Baltimore: Johns Hopkins University Press. 1973. Pp. x, 163. Cloth \$7.50, paper \$2.95.

This is a novel approach to the analysis of contemporary Israeli foreign policy, one seeking to discern the historical roots of such policy in ancient, prestate Zionist, and early independence periods.

The book opens with a brief note on the foreign policy of the ancient Jewish kingdoms of Judah and Israel, which the author believes represents for current Israeli leaders more than mere nationalist nostalgia; rather, it constitutes an integral part of their "historical memory" that directly affects foreign policy making. In broad outlines the international political circumstances besetting those two hapless mini-monarchies of antiquity indeed resemble those affecting the Israel of today. They, too, were caught in the vortex of an intraregional conflict whose poles were in the Nile and Euphrates valleys; and they also relied on a combination of diplomacy, military strength, and great-power patronage to eschew national disaster. However, some of Roberts's more fanciful comparisons, such as that between the Israelites' desert wanderings in search of a homeland and the early Zionist movement's brief flirtation

with the idea of a temporary shelter in Uganda, remind one of the perils of pushing historical analogies too far.

It is precisely this reliance upon *Realpolitik*, which Roberts reminds us is a "universal tendency of states," rather than upon some undefined Judeo-Christian ethical premises that the author finds to be the leitmotiv of Israeli foreign policy from ancient through Zionist and Israeli eras and that he finds morally objectionable.

Despite the fact that this objection is a main theme of the work, the author never adequately clarifies the source of his moral indignation. He states in the introduction that by behaving as any other territorial actor and by repressing ethical considerations when the security of the state is imperiled, Israel has been "in conflict with the moral aspirations of at least some prominent Zionist thinkers, as well as with the claim of Israel's apologists" (p. viii). Yet even the most conciliatory Zionist thinkers, including some of the binationalists Roberts mentions, would be hard pressed to place abstract ethical principles above national survival, without which ethical imperatives are impossible to implement. Why the Jewish state then, which Roberts concedes is based on a legitimate nationalist movement, should be judged by a higher standard of international morality than that to which she is subjected is never made clear.

Turning back from the realm of ethics to that of foreign policy, Roberts discusses "Israel's imperialism," a modified, nonannexationist type motivated by external hostility and internal pressures and limited by vestigial ethical as well as more pragmatic geopolitical considerations. Summarizing Israel's foreign policy objectives Roberts includes, first, the maintenance of a favorable military balance of power vis-à-vis the Arabs; second, the physical control over certain strategic points; and third, the preclusion of Arab unity against Israel. The author sees these limited, essentially defensive goals as basically compatible with those of the other major imperialistic regional power, Egypt, which seeks positive political, cultural, economic, and psychological domination over the Middle East. And given what he believes is the two superpowers' desire to disengage from the region, he foresees an Israeli-Egyptian agreement whereby Egypt will accept Israeli overlordship in return for Egyptian domination over the Arab Middle East.

I dare say that this denouement—ignoring for the moment the traditional Islamic and more

modern Arab nationalist constraints on Egypt—suffers from the same idealism, moralism, and optimism that the work, however sincere, exudes throughout. Something midway between mere survival and regional hegemony, some mutual recognition of and by equals, is the only viable long-term option for Israeli foreign policy, one that would satisfy both the idealists and the "Realpoliticians."

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## AFRICA

ADEKUNLE AJALA. *Pan-Africanism: Evolution, Progress and Prospects*. New York: St. Martin's Press. 1973. Pp. xiii, 442. \$10.95.

The popularity of the term "pan-Africanism" can be traced to the pioneering pan-African conference of 1900 held in London. Since that time the term has been applied to two related concepts, namely solidarity among African peoples worldwide and political cooperation on the African continent itself. The present study is devoted mainly to the latter concept.

The book is divided into three parts. The first attempts a rapid overview of the pan-African movement from 1900 to the 1960s. It skips lightly over the 1900 conference, Marcus Garvey's University Negro Improvement Association, the pan-African congresses of W. E. B. Du Bois, and a few other landmarks along the way. The section includes an extended discussion of the Organization of African Unity founded in 1963 by African heads of government and ends with biographical sketches of the "torchbearers" of the movement (Garvey and Du Bois), the "progressives" (Kwame Nkrumah and Sékou Touré), and the "moderates" (among them Léopold Senghor and Haile Selassie).

Part 2, which comprises the bulk of the book, is a collection of case studies illustrating major successes and failures in the quest for cooperation. The studies are drawn mostly from the 1960s, the decade in which most African states regained their independence from Europe. The major successes seem to have revolved around diplomatic moves by African leaders to solve boundary disputes and other assorted problems. The author identifies among major failures the collapse of several attempts at economic and political integration, the failure of African mediation to solve the Congo crisis or the Nigerian civil war, the dangerous flirtation with

South Africa on the part of some governments, and the reluctance in many quarters to render unequivocal support to the Arab cause in the continuing Middle East crisis. In part 3 the author states his own case for pan-Africanism.

The book, though in no way a source of major new information, is nevertheless a useful guide to intra-African relations over the last decade and a half. For the most part the author's style is unpretentious but very lucid. Lucidity, though, breaks down on occasion (for example, in the biographical sketches of Touré and Senghor).

The author puts forward several proposals for more effective African unity. He favors increased reliance on intra-African solutions to African problems, is suspicious of foreign aid, suggests withdrawal from what he calls "Eur-African" organizations (such as the European Economic Community), and bemoans the gullibility of some African leaders. He advocates an African federation comprising fourteen states with a two-chamber parliament and only two political parties. He proposes a federal civil service with quotas for each country and an African lingua franca.

Despite clear evidence contained in the book that basic differences in ideological orientation militate against this type of comprehensive unity at present, he does not address himself to this question in his proposals for a United States of Africa. He seems to think that good, honest leaders will solve the problem. But the best of leaders may disagree concerning the kind of social relations that will exist within their states.

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MERVYN HISKETT. *The Sword of Truth: The Life and Times of the Shehu Usman dan Fodio*. New York: Oxford University Press. 1973. Pp. xxii, 194. \$7.50.

The assumption that Africa before colonization was an intellectual and ideological *tabula rasa* has already been seriously challenged by Africanists over the past decade. Professor Hiskett in this masterful, first-published biography of Shehu Usman dan Fodio shatters that assumption in regard to Hausaland in the late eighteenth and early nineteenth centuries.

The Shehu, who was then at the center of a revolutionary movement in the northern region of present-day Nigeria, is brought back to life under the very eyes of the reader. Indeed,

Hiskett skillfully reconstitutes the environment that witnessed dan Fodio's rise from childhood to maturity. Then he describes the life and deeds of that African statesman and saint using oral tradition and manuscripts both in Arabic, the language of the literati of that period, and in the vernacular of Hausaland. On the other hand, to make the book accessible to readers unfamiliar with the history of Islam and of West Africa he artfully blends in related religious and historical factors and interprets motives and *démarches*.

*The Sword of Truth* is a beautiful example of scholarly history made relevant and interesting. The book is well conceived, and all its parts are equally important and further illumine the history of the area. Sufism in the Western Sudan is discussed in the context of dan Fodio's personal mystical experiences. The discussion of the jihad's motives as well as the prosecution of military campaigns culminating in the establishment of the Fulani empire are welcome contributions to the study of the pre-Nigerian period. But what I am most impressed with is the treatment given the Shehu's intellectual (theological, ideological, and juridical) personality and legacy. This is an indispensable book to West Africanists as well as a pleasant one to read.

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G. WESLEY JOHNSON, JR. *The Emergence of Black Politics in Senegal: The Struggle for Power in the Four Communes, 1900-1920*. Stanford: Stanford University Press, for the Hoover Institution on War, Revolution and Peace. 1971. Pp. x, 260. \$8.75.

The political movements that in our generation liberated most of Africa are often seen as coming rather suddenly after 1945. That was certainly the perspective of the Europeans who were surprised at the intensity and swiftness with which Africa achieved independence. To some degree this view was also shared by the nationalists themselves who, in an effort to exaggerate their own importance, tended to ignore or deny the contributions of their predecessors. Professor G. Wesley Johnson in this fine book reveals the extent to which a political consciousness developed in Senegal prior to 1920.

Politics in nineteenth-century Senegal revolved around the colonial councils, the municipal councils, and the elections for deputy to

the French Chamber of Deputies in Paris. These institutions developed early in the French trading town of Saint Louis and on the island of Gorée, and subsequently they spread to Dakar and Rufisque, which also were granted commune status. All the people living in these four communes were eligible to vote on the same basis as French citizens in France. Political power was shared between the colored population (Johnson calls them Creole) and the white traders.

By 1900 the black electorate, proud of its position in the French empire, had become increasingly self-assertive. The special rights of the *originaires*, as the inhabitants of the four communes were called, were at times ignored, and after 1900 they were increasingly challenged by the French administration. The *originaires* in turn developed concern for safeguarding their rights, and some even wanted to see citizenship extended to the rest of the African populations in the French West African Federation. The citizenship of the *originaires* was unclear and was only confirmed by laws in 1915 and 1916.

The man who legally secured the rights of the *originaires* was Blaise Diagne, the first black African deputy to sit in the French parliament. He was elected in 1914 by an appeal to black racial solidarity. The numerical superiority of the black population was now galvanized into political power. In Paris Diagne skillfully appealed to the French need for more black troops and got the wartime parliament to pass the laws clarifying the citizenship of the *originaires*. These laws increased Diagne's popularity, and in 1920 all political institutions in the four communes fell under the control of his "party."

Johnson's study illustrates very well that the politics of the four communes were not insulated from the rest of the colony. On the contrary; Diagne, for instance, received much of his financial support from the inland, particularly as represented by the Islamic Mourid brotherhood. Many of the young men around Diagne equally shared a concern for the inland and maintained an important network of political connections. The style of politics and the rights to municipal government and French citizenship that the *originaires* developed became the model for the rest of French West Africa and after World War II were implemented there.

This very fine study on the emergence of black politics in Senegal will be followed by another volume delineating the period up to

World War II and thus completing the history of the origins of modern Senegalese politics.

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Bloomington

WILLIAM B. COHEN. *Rulers of Empire: The French Colonial Service in Africa*. (Hoover Institution Publications 95.) Stanford: Hoover Institution Press, Stanford University. 1971. Pp. xv, 279. \$9.50.

JEAN SURET-CANALE. *French Colonialism in Tropical Africa, 1900-1945*. Translated from the French by TILL GOTTHEINER. New York: Pica Press; distrib. by Universe Books, New York. 1971. Pp. xvii, 521. \$17.50.

The works of William B. Cohen and Jean Suret-Canale make very real contributions in different ways to our understanding of French colonial rule and its legacy in West and Equatorial Africa. Cohen is the first scholar to undertake a comprehensive study of the development of the French colonial service in Africa from the mid-nineteenth century to the post-independence era. His sources include published materials and the personnel files of 1,000 colonial administrators from the archives in Paris and Dakar. In addition he employed a questionnaire, which was completed by over 250 officials, and held many interviews.

Cohen's findings contain no real surprises; rather they solidly document hypotheses already held by many specialists. Among his most important conclusions are that, first, until well after the First World War and in quite a few areas until after the Second World War, the colonial administrators in Africa and not the officials in Paris were the real rulers of the empire; second, the administrators contributed significantly to the destruction of the pre-existing African political and social structures; and third, the colonial administration of the post-1914 period established the bureaucratic structures that the independent African states have maintained with but few changes. Cohen's study is well organized, clearly and colorfully written, and illustrated with photographs of important colonial administrators. It has been translated into French in the Mondes d'Outre-Mer series of Éditions Berger-Levrault.

Till Gottheiner's English translation of Suret-Canale's book is not based upon the original edition (1964) but upon a subsequent revision (1966). Some of the translator's errors betray ignorance of French life and African history. For example, *état civil* is rendered as "civil



service" and the *originaires* of Senegal are called "originators." Numerous less-serious translation errors, textual gaps, and transpositions, along with inconsistencies in the spelling of names ("Lebou," "Leboue," and "Lebon" for the original people of Dakar), give further evidence of the translator's and editor's incompetence or carelessness. The many errors do a real disservice to the author and his English-language readers.

Jean Suret-Canale first published the present volume as the second of what has become a three-volume series on French colonialism in Black Africa. The first volume treated African geography and peoples before turning to the French penetration and occupation of Africa before 1900. The third volume studied the era of decolonization (1945–60). Volume 2 concentrates on the period of colonial rule from 1900 to 1945. Within this time span the author sees the period between the two world wars as the apogee of European colonialism and the wars themselves as contributing profoundly to the awakening of African peoples against the colonial system. Except for some archival work in Guinea Suret-Canale relies on French secondary sources and rarely uses the available English-language materials that might further enrich his study.

Colonialism for Suret-Canale results from the overseas expansion of the capitalist system and leads to economic exploitation, which is accompanied by political, social, and cultural oppression. He piles up a tremendous amount of data on the mechanisms and consequences of the colonial regime. He shows how colonialism destroyed irrevocably the earlier institutions of Africa and made the African territories economic appendages of France. The colonial era becomes, therefore, not just an interlude of direct European rule but an irreversible experience that has brought Black Africa into global systems dominated by the industrialized nations of the West.

Jean Suret-Canale served as a teacher and an academic administrator in Senegal and Guinea for various periods between the 1940s and early 1960s. At different times he suffered loss of employment and expulsion as a result of his anticolonialist activities. He writes from the perspective of one who has personally experienced the evils of French colonialism in Black Africa. His work also reflects strong Marxist and nationalist biases, which take the form of unrelenting hostility toward capitalism, Christianity, and the United States. These tendencies mar his work and sometimes lead to simplistic

analyses. Yet despite its shortcomings, Suret-Canale's work provides an indispensable introduction to French colonial rule in West and Equatorial Africa. For West Africa it might be read jointly with the less-detailed but more balanced study by Michael Crowder, *West Africa under Colonial Rule* (1968).

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JON WORONOFF. *West African Wager: Houphouet versus Nkrumah*. Metuchen, N.J.: Scarecrow Press, 1972. Pp. xii, 357. \$10.00.

In 1957 a wager was made between Kwame Nkrumah of Ghana and Felix Houphouet-Boigny of the Ivory Coast. Each claimed that his country would have outstripped the other's by the end of a decade. Woronoff's latest book considers this wager and compares the experiments that followed it. Sections deal with the struggles of the two countries for independence, internal political programs, foreign policies, economic development, and, in a chapter called "Daily Life," with a vaguely sociological grab bag of "tribalisms," class characteristics, and welfare and education policies.

In many ways Ghana and the Ivory Coast are well suited to such a comparison. Essentially similar in social structure, environmental conditions, and the stage of development at independence, the difference in their progress since then is particularly striking. Nkrumah's government attempted a sweeping transformation of Ghanaian social institutions and economic life. An interventionist foreign policy was coupled with state socialism, the guiding ideology for domestic reform. By contrast, the focus of Houphouet-Boigny's policy has been on the gradual improvement of the predominantly agricultural economy of his country. His economic programs have been fiscally orthodox, and accommodation rather than confrontation has characterized his politics. Gradualism has been his catchword, "liberal capitalism" his credo.

The results of these contrasting approaches to political development are deceptively easy to balance. By most economic measures the Ivory Coast has performed brilliantly, while Ghana has one of the most dismal records in the third world. Many observers have an ideological heyday explaining and comparing these two experiments in the language of traditional cold-war models. The wager itself has encouraged simplistic analysis, and Woronoff has fallen into such a trap at the conclusion of his generally

useful book. Houphouët is resoundingly identified as "The Winner," and it is in such facile conclusions and in some of the author's sweeping generalizations, often without benefit of cited sources (such as those in "Daily Life"), that his book is weakest. Furthermore, although he writes that this study has been "waiting for an author ever since April 6, 1967," Woronoff does not include recent work on this subject in his bibliography. The study could have profited from an awareness of Philip Foster's and Aristide Zolberg's *Ghana and the Ivory Coast* (eds., 1971), where Elliott Berg persuasively argues against the premature drawing up of balance sheets for the period since independence. Woronoff's contribution in this volume is his succinct narrative of aspects of the recent economic and political history of these two West African countries.

CHARLOTTE A. QUINN  
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DAVID WILLIAM COHEN. *The Historical Tradition of Busoga: Mukama and Kintu*. (Oxford Studies in African Affairs.) New York: Oxford University Press. 1972. Pp. x, 218. \$12.75.

The use of oral tradition as a sound methodological tool in reconstructing the precolonial histories of African societies has steadily gained acceptance from an initially skeptical audience over the past couple of decades. Dr. Cohen, with this scholarly and searchingly analytical piece of work, should make that acceptance nearly universal. Concentrating on the traditions surrounding the Adamic heroes Kintu and Mukama, who dominated the crucial period of the foundation of the sixty-eight states that ultimately became known as Busoga, Cohen has managed to produce one of the finest reconstructions of traditional East African history it has been my pleasure to encounter. It must be emphasized that the criticisms that follow are minor ones and are in no way intended to detract from the real contribution Cohen has made.

While one should be extremely careful of criticizing the practical methodology of another oral historian (especially a methodology that successfully elicited the volume and the quality of data that Cohen's did), it seems somewhat peculiar that Cohen elected to terminate his field research nearly a year ahead of schedule, notwithstanding his feeling "that the research was at the point of diminishing returns." Surely the additional time could have been used for more intensive interviewing of some of

those areas in which Cohen employed questionnaires, a device that most oral historians agree is far less satisfactory than a formal interview. Also with regard to methodology, a list of informants, or at least specific biographical data on some of the primary informants, would have been in order, despite the assurance (p. 38) of a forthcoming volume of Soga texts.

I thought that some of Cohen's terminology, such as the outdated "Nilo-Hamites," the puzzling "Kalenjin Cushitic speakers" (p. 95), and the use of the misleading and strictly inappropriate "pastoralists" to describe the Lwo speakers, was unfortunate. On rare occasion, some of Cohen's analysis seemed open to question. One is a bit surprised, for instance, that he does not question the suggested importance of bows in Lwo hunting activities or that he does not actually explore the possibility of contacts with a Maasai (or other Southern Paraniotic-speaking group), as indicated by the tradition on page 91.

As noted above, such flaws are minor and in no way detract from Cohen's achievement as a whole. Space would not permit one even to mention all that is outstanding in this work. Nevertheless, the first third of the book (where methodology and the nature of the oral data are discussed), the frequent reference to current archeological findings, the remarkably thorough treatment of pre-Lwo groups in northern Busoga, and the excellent chapter on the Lwo "camps" at least must be mentioned in passing as being especially successful. The book has added valuable new information on both Busoga and, of equal importance, on the "wider region" of the Interlacustrine area as a whole.

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CARLO ZAGHI. *L'Europa davanti all'Africa*. Volume 1, *La via del Nilo*. [Naples:] Edizioni Cymba. 1971. Pp. viii, 628. L. 13,000.

Carlo Zaghi is one of the grand old men of Italian colonial history. He has been a prolific writer on that topic and on the nineteenth-century European penetration of Africa. In the heyday of Italian imperialism his articles appeared in all the major Italian periodicals devoted to colonialism. His first book, published forty years ago, dealt with the genesis of Italian imperialism in the Red Sea (*Le origini della Colonia eritrea* [Bologna, 1934]), while the second was a biography of one of the Italian adventurers who served under Gordon in the

Sudan (*Vita di Romolo Gessi* [Milan, 1940]). Subsequently he published an important monograph on the Sudan (*Gordon, Gessi e la riconquista del Sudan, 1874-1881* [Florence, 1947]) and two significant works on major stages in the development of Italy's interest in Ethiopia (*P. S. Mancini, l'Africa e il problema del Mediterraneo* [Rome, 1956] and *Crispi e Menelich nel diario inedito del conte Salimbeni* [Turin, 1957]). Even a recent excursion into European history (*Napoleone e l'Europa* [Naples, 1970]) contained material of no little importance for the study of Europe's involvement in Africa. Most recently Zaghi, who holds the chair in modern history at the University of Bari and the chair in African history at the Istituto Universitario Orientale in Naples, has turned his efforts to works of broader scope. Thus, on the one hand, he has tried to cast his studies of Italian imperialism in the broader context of European history (*L'Africa nella coscienza europea e l'imperialismo italiano* [Naples, 1973]), while on the other he has indicated in *L'Europa davanti all'Africa* his major commitment to producing a seven-volume historical study of European-African relations.

If *La via del Nilo*, the first volume of the projected series, is any indication, then Zaghi will be making a major contribution to the fields of colonial and African history. This massive volume, replete with three dozen effective illustrations and eleven clear maps, has a scholarly apparatus that impressively conveys the author's mastery of both the primary materials found in the archives of London, Paris, Rome, Turin, Vienna, and Cairo and the growing literature of African history written by his contemporaries. This is no attempt at a justification of Italian activities in a narrow national context but a work of monumental proportions that should be called to the attention of all fellow scholars for whom an English translation would be most helpful. In the thirteen chapters of this volume Zaghi first sets the stage with a study of the Barbary states and Mohammed Ali's Egypt. He then places the first Egyptian conquest of the Sudan in the context of Egyptian economic and political history before devoting three chapters to the Sudan's economy, its exploitation by Egyptian administrators and European and Arab slave traders, and the position of the southern Sudanese, caught as they were between the Europeans and the Egyptian "Turks." After defining the role of Egypt and Africa in the India policy of Great Britain, Zaghi then deals with the Egyptian, British, and Italian interest in the

control of the Nile. This leads him into a discussion of the role played by Romolo Gessi in the attempt to suppress the Sudanese slave trade. Subsequent chapters deal with Gordon at Khartoum, Europe and "the idea of Africa," and the search for the sources of the Nile. The final chapter delineates the career of Giovanni Miani, an explorer little known outside Italy, and his polemic with Speke; much new information is introduced from Miani's unpublished journals.

If this work has one fault, it is that it is more a series of extremely well-documented and descriptive studies of nineteenth-century history than an analysis of the era with a well-defined theme or a demonstrable thesis. This criticism may be somewhat unfair, given the author's nearly encyclopedic scholarship and the fact that this is but the first volume in a series that may very well attempt the kind of synthesis which would be welcomed by all serious students of Africa. A second volume, *I russi in Etiopia*, has already appeared, but by its very nature it is limited to a study of tsarist involvement in Ethiopia. The publisher has indicated that the third volume will consist of hitherto unpublished diaries, journals, and memoirs of travelers, merchants, and explorers on the Upper Nile from 1840 to 1860, including works by Miani, Vayssière, Brun Rollet, Knoblecher, and De Bono. All these will make a useful contribution to the literature of African and colonial history. The remaining volumes, concerned with the Portuguese in East Africa, the slave trade, and the political and essentially economic reasons for European success in putting Africa in a subservient relationship, are awaited with considerable interest and expectation.

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IAIN R. SMITH. *The Emin Pasha Relief Expedition, 1886-1890*. (Oxford Studies in African Affairs.) New York: Oxford University Press. 1972. Pp. xiii, 335. \$21.00.

The Emin Pasha relief expedition, Henry M. Stanley's last great African adventure, began in the well-publicized humanitarian image of Livingstone's journeys to spread Christianity, commerce, and European civilization. It ended amid damaging and sensational public recriminations that dulled European enthusiasm for the exploits of free-lance explorers and confirmed Stanley's reputation as the most egotisti-

cal and the harshest of the late nineteenth-century adventurers in Africa. Dr. Smith has provided a painstakingly researched and well-written narrative of Stanley's progress toward the Upper Nile where he was to contact Emin Pasha, the isolated governor of khedivial Egypt's southernmost province of Equatoria cut off from the outside world since the rise of the Mahdist state in the southern Sudan during 1883-85. Smith skillfully dissects the personalities, diplomacy, duplicity, and greed, as well as the sensitivity and honor, that comprised the anatomy of European exploration everywhere in the world.

Smith's thoroughness and sensitive interpretations convincingly explain the events on the Upper Nile that other historians, following Langer, have until very recently characterized as "simply baffling [with] the many contradictions on all sides only serv[ing] to enhance the confusion." The author has supplemented conventional diplomatic sources with the numerous and contradictory accounts left by the expedition's European leaders, by Emin Pasha, and by assorted literate Africans including the famous Afro-Arab Tipoo Tib. The resulting narrative details the confused genesis of the expedition in a welter of sponsors with conflicting goals, the hardships resulting from overly optimistic planning that it faced as it moved up the Congo River, and its consequently depleted condition when it finally arrived on Lake Albert, more in need of than able to offer relief. The expedition's arrival unexpectedly thwarted the purposes of its backers by provoking a mutiny among Emin's Egyptian troops in Equatoria, made its major accomplishments in charting the geography of the Ruwenzori Mountains, and ended anticlimactically as Stanley and Emin finally emerged in German East Africa with little but failure to report to their original backers.

More than a straightforward narrative, the book offers a subtle analysis of the dynamics of African exploration and the effects of isolation, hardship, enforced intimacy, and ignorance on Stanley and his companions. Smith generally praises Emin's conduct, concurs with his judgment that Stanley behaved in a self-righteous and arrogant manner, and acquits Tipoo Tib of Stanley's charges of perfidy in failing to supply carriers for the expedition at a crucial juncture. The extended account of events in Equatoria between 1883 and 1889 greatly expands the sketches previously available. There are descriptions of Stanley's imperious and often cruel treatment of Africans but relatively little

emphasis on this and other "Africanist" themes. But the perspective is decidedly not Eurocentric either because Smith has forsaken the traditional debates of European diplomatic history to concentrate on the expedition's internal history and its relationship to its surroundings. For imperial historians the book contributes insights into the pressures generating the aggressiveness of the "men-on-the-spot" who repeatedly ignored European ministers' pleas for restraint. For Africanists it provides understanding of how inhuman practices paradoxically emerged from popular humanitarian impulses during the age of European exploration in Africa.

JOSEPH C. MILLER  
University of Virginia

BRIAN ROBERTS. *Churchills in Africa*. New York: Taplinger Publishing Company. 1971. Pp. xiv, 370. \$8.50.

Brian Roberts, an English-born writer who now resides in South Africa, has written a readable and entertaining book about the adventures of a trio of Churchills in South Africa during the 1890s. Lord Randolph was the first upon the scene when in 1891, at the time of growing tensions between the Afrikaner republics and British imperial and subimperial interests, he undertook a trip to the British South Africa Company's territory of Mashonaland (modern-day Rhodesia), which also involved travel in the Cape Colony and the Transvaal. This visit, which became a highly controversial one, was part of an overall effort to revive his once-brilliant political career, but it failed in its purpose, largely because he was already an ill man (he died in 1895). The second member of the trio was Lady Sarah Wilson, Lord Randolph's sister and Winston Churchill's aunt, who first went to South Africa in 1895 and by chance became privy to some of the plotting that led to the Jameson Raid. In 1899 she accompanied her soldier-husband to South Africa and ultimately ended up as a participant in the siege of Mafeking, from which she emerged as a popular heroine and, in the author's estimation, perhaps the most celebrated woman in England at that time. Winston Churchill came to South Africa in 1899 as a war correspondent determined to conduct himself in a way that would gain widespread public recognition of a sort that would further his political career. In this he was successful, for his highly publicized escape from a Boer prisoner-of-war camp meant that henceforth



he was known in his own right and not as Lord Randolph's son.

While the sojourns of the Churchills in South Africa have an important place in their lives, from the perspective of South African history they form but a minor episode in a turbulent era. The contribution of this book thus must be considered in terms of its insights into the Churchills. Here Roberts has not covered new ground, but he has pointed out and called attention to the importance of South Africa to the Churchills at a crucial juncture in both that country's and the family's history. He has also demonstrated that Sarah Churchill Wilson merits attention on her own account and not simply as a close female relative of a famous father and his yet more famous son. Roberts's approach to his topic, however, precludes him from making a substantive contribution to our knowledge of the Churchills. For him, their South African adventures represent a good story worth the telling rather than a study of members of a prominent late Victorian family that could reveal important insights into a turning point in the history of both the British empire and South Africa.

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University of Florida

ALAN PATON. *Apartheid and the Archbishop: The Life and Times of Geoffrey Clayton, Archbishop of Cape Town*. New York: Charles Scribner's Sons. 1973. Pp. xiii, 311. \$10.00.

In his biography of Jan H. Hofmeyr, published in 1964, Alan Paton examined South Africa's racial problems from the point of view of a liberal Afrikaner politician. In this volume he continues this examination from the point of view of the experiences of Geoffrey Clayton, a leading English-speaking churchman. Hofmeyr died in his prime, shortly after the white electorate in 1948 rejected the mild liberalism for which he stood; Clayton, the nonpolitical archbishop, died of a heart attack in 1957 moments after he had signed a letter to the prime minister declaring that if the bill which would restrict the right of Africans to attend churches in "white areas" were to become law, he would be unable to obey it.

Clayton came to South Africa in 1934 as bishop of Johannesburg and became archbishop in 1948. In his determination to be a loyal South African, he did not fit the Afrikaner's image of the typical clergyman of "die Engelse kerk." Averse to being drawn into politics, he was not sympathetic with activists

like Bishop Ambrose Reeves, Trevor Huddleston, and Michael Scott. But when aroused the archbishop was a match for the best.

An Anglo-Catholic, Clayton did not believe that an understanding between the Anglican and Dutch Reformed churches was possible as their theologies were incompatible. Paton disagrees with that judgment. He holds that theology, race, and history are all responsible for the division between the Afrikaner and the English-speaking South Africans, history being the most important factor with the proviso that the theological and racial factors are inseparable from it. I am of the opinion that Paton's view needs some correction. There is nothing in classic Calvinism to support racialism. Reformed churches throughout the world have condemned apartheid. Afrikaners pervert their theology to justify their practices, while Anglicans maintain a correct theological position but ignore it in practice. It is the same human factor that accounts for both aberrations. Moreover, the Reformed churches are a folk church—the church of a people who harbor bitter historical grievances and memories of a long, hard struggle against Africans and Britain.

Archbishop Geoffrey Clayton was a great preacher, an able church administrator, a pungent personality—a remarkable man. Paton's treatment of him is sympathetic but not uncritical. The volume is a worthy companion piece to his biography of Hofmeyr. The two studies illuminate the recent history of an unhappy land.

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## ASIA AND THE EAST

GILBERT ROZMAN. *Urban Networks in Ch'ing China and Tokugawa Japan*. Princeton: Princeton University Press. 1973. Pp. xiv, 355. \$16.50.

It must be noted at the outset that Gilbert Rozman has undertaken a task never before attempted. His aim is to measure in concrete quantitative terms the growth of urbanization in both China and Japan until the modern period. (The word "modern" is used in this context to refer to the period after 1800 when strong exogenous influences began to make themselves felt.) By comparing the rate and pattern of growth in two separate premodern societies, Rozman proposes to relate urban growth to the differing social and political histories of both societies. He also informs us that this book is the first part of a larger work



designed to compare urban growth in China and Japan to that of other premodern large-scale societies.

In carrying out this study Rozman has drawn on an impressive array of primary and secondary sources in Chinese and Japanese as well as on what is available in Western languages (including Russian). He has also demonstrated his firm control of contemporary sociological and "urbanological" literature.

The book is densely packed with statistical data, with comparative tables, charts, and long passages full of descriptive data concerning characteristics of "urban networks" in the two countries. While the author allows himself passing reflections on the social and political significance of the differing developments in both societies, on the whole, he adheres relentlessly to his task of quantifying urban growth, particularly in its relationship to commercial development. There are, indeed, long passages such as the chapter on Chihli Province in China and Kantō region in Japan that read almost like a handbook of useful data. The book will furnish food for discussion among the statistically inclined for some time to come. The author himself admits that the figures are estimates and "must be used with caution," and many will challenge his statistical reasoning on many matters, yet there can be no doubt that he has opened up the subject.

In this brief review I do not propose to challenge Rozman's figures. Indeed, to one layman, the specific conclusions he draws from his figures seem eminently plausible. In discussing the origins and earlier history of urban centers he stresses the role of the politico-administrative (also ceremonial-ritual) function. The emergence of "central places" as marketing centers becomes important only in the latter centuries of Chinese and Japanese history. Of course, the administrative centers themselves become enormously important centers of commercial growth.

For purposes of analysis, Rozman divides urban settlements into seven levels ranging from national administrative centers to "intermediate and standard marketing settlements." While he does not dwell on this point, I for one was struck by the overwhelming role of deliberate "administrative" policy in the urban history of both societies.

A concept particularly stressed by Rozman is the concept of "urban networks." The history of separate cities should not be considered in isolation. The spatial arrangements and concentration of urban centers in various regions

must be viewed as a function of many interdependent variables—geographic constraints, marketing patterns, communication networks, and political-military factors. Presumably, such differences in "organization in space" have something to do with factors such as the degree of separation of urban from rural life and the degree to which urban or rural patterns "set the tone." These patterns may relate—it is implied—to certain general laws of societal development.

Among the more striking conclusions that emerge from the work are that by 1800 the proportion of urban to rural population in China and Japan had probably outstripped the world average; that by 1800 Japan had far outstripped China in terms of urban-rural ratios (16% versus 6.7%), even though its earlier development had been much slower; and that "in China and Japan at least 40 percent of all city inhabitants prior to 1850 lived after 1600." In terms of urban growth, Ming-Ch'ing China and Tokugawa Japan were (in premodern terms) the most dynamic societies in the world.

In this first volume of his project, Rozman has imposed upon himself a severe self-denying ordinance. He does not spell out many of the cultural and social implications of his findings even though it is strongly suggested that urban growth provides a key to some universal process of "societal development" cutting across all difference of culture. If Rozman had attempted to spell out the "necessary" cultural and social consequences of his stages of urban growth either for the inhabitants of the cities themselves or for the society as a whole, he would certainly have written a much more controversial book. To the extent that he confines his attention to the quantitative growth of the urban sector as such, he has, I think, made an impressive case.

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MICHAEL H. HUNT. *Frontier Defense and the Open Door: Manchuria in Chinese-American Relations, 1895-1911*. (Yale Historical Publications. Miscellany, 95.) New Haven: Yale University Press. 1973. Pp. xiv, 281. \$12.50.

The title of Michael H. Hunt's volume, *Frontier Defense and the Open Door*, concisely summarizes his major themes. These are that the idea of promoting American investment in Manchuria must be understood first as a facet of the ancient but newly adaptive Chinese

policy of frontier defense and second as part of the development of the so-called Open Door policy of the United States.

Hunt is at his best in treating the issue of Manchuria in Chinese history. He has provided a valuable account of the efforts of officials such as Yuan Shih-k'ai, Hsu Shih-ch'ang, and Tang Shao-i to safeguard the Manchurian provinces from Russian and Japanese encroachment by obtaining American loans for improved transport, better administration, and a Manchurian bank. Hunt argues convincingly that many of these projects were Chinese, not American, in conception. His discussion of the role of Hsi-liang in reviving the Chin-Ai railroad scheme is particularly helpful. His general thesis should encourage needed revision of simplistic accounts of American "dollar diplomacy."

While Hunt lauds the Chinese officials, perhaps to excess, and slights the pernicious features of the Manchu regime, he vigorously probes the "misconceptions and prejudices of American statesmen." Certainly, United States envoys such as Charles Denby and Edwin Conger had their faults, and the importance of Willard Straight has been exaggerated. But his indictment of virtually every American official reveals a distinct bias. His criticism of the United States for indifference to emerging Chinese nationalism is unfair. He is not sufficiently skeptical about the significance of the Open Door notes, making more of John Hay's maneuvers than did the secretary himself. He also overplays the role of Brooks Adams, A. T. Mahan, and Albert Beveridge.

Nor does Hunt explain why Americans sought regulation of ports in China. He argues without substantiation that President Theodore Roosevelt was interested in Manchuria for commercial reasons and generally undervalues Elihu Root's characteristic prudence and restraint. He likewise fails to appreciate why neither Roosevelt nor Root placed much faith in China.

His harsh treatment of the Taft-Knox administration is equally inadequate. He does not see that their policy marked a studied shift from Roosevelt's balance of power to the familiar idea of concerted action and was not altogether the result of the prejudices of Knox's young associates. Moreover, because Hunt ceased his analysis in 1911, he does not bring out that the cooperative policy espoused by Knox attained a measure of success in the subsequent two years.

Despite some marked shortcomings in the analysis of American policy, this volume is instructive for that purpose and worthy for its

judicious treatment of the neglected Chinese side.

PAUL S. HOLBO  
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KRISHAN BHATIA. *The Ordeal of Nationhood: A Social Study of India since Independence, 1947-1970*. New York: Atheneum. 1971. Pp. vi, 390. \$10.00.

*The Ordeal of Nationhood* is a readable, perceptive, and disarmingly frank account of India's achievements and failures since independence, written by a veteran journalist of *The Hindustan Times*. His account is especially interesting when it deals with India's current political leadership, the nature of the decision-making process in the higher echelons, the balancing of conflicting interests among the political elite, and the role of pressure groups and lobbies of one kind or another. Like some other recent works covering the same period of Indian history, it puts forward a revisionist assessment of Nehru as a charismatic leader with feet of clay, leaving the reader to wonder how, if Nehru's personality was such a compound of indecision and prevarication, he ever managed to gain and retain his hold over a nationalist movement in which there was no shortage of talent or ambition.

Looking to the future, Bhatia's outlook is distinctly pessimistic, although perhaps for the wrong reasons. It is not that he underestimates the positive achievements of the postindependence period but that he is too preoccupied with what he regards as ineffective leadership at the top. This appears to be the consequence of an attitude toward both past and present that emphasizes the need for "great men" to give direction to their age. He sees no great men—or women—in the India of today, and herein lies the principal source of his despondency. He scarcely touches upon what is a central issue in India, as it is in much of the third world: the fact that after a quarter of a century of independence and rising expectations, the rich are getting richer and the poor are getting poorer. In retrospect, the transfer of power in 1947 can be seen largely in terms of a political transformation, with white sahibs replaced by brown ones, a change that involved no revolutionary restructuring of society. It is probable that in the next decades this lacuna will become the dominant issue throughout the subcontinent, calling for qualities of leadership rather different from those revered by Bhatia.

Although the author has little to say to the

professional economist or social scientist who specializes in South Asian affairs, and although he touches rather superficially on historical matters, he has written an admirable account of recent developments in India, as seen through the eyes of a comparative insider. This is a book to be added to the shelves of those libraries which can only aspire to selective holdings on South Asia. It could be usefully included as the final title on an Indian history survey course for undergraduates.

GAVIN R. G. HAMBLBY  
Yale University

LOUIS DUPREE. *Afghanistan*. Princeton: Princeton University Press. 1973. Pp. xxiv, 760. \$22.50.

"Primarily a preliminary statement after twenty years of research and intimate relations with Afghanistan and surrounding areas," Dupree writes, "this book attempts the impossible: to present a study of a single piece of real estate through time, from one cell up, from the Stone Age to tomorrow." He adds, "This is an attempt by an anthropologist to ferret out the patterns, functional and dysfunctional, in the total synchronic-ecological-cultural sense."

Whatever these statements may really mean, the book emerges as a conglomeration of chapters and sections, some quite long, some very short and consisting mostly of charts. He has four main sections, "The Land," "The People," "The Past," and "The Present," this last covering the period from 1880 to roughly 1971. There is a series of appendixes, including domesticated plants and animals, calendars, folk music and instruments (mostly illustrations), and Afghan cabinets, 1963-71. There is also a bibliography of works cited and an epilogue in which he discusses briefly the overthrow of the monarchy in July 1973.

It is impossible to deal with his treatment of such a vast range of subjects as this in so short a space, and it is probably not necessary, for only certain sections of the book will have to be read with any seriousness. Certain parts of the work, which are outside the author's own professed sphere of competence, are not going to be of much use, either to the general reader or the specialist in search of information.

The section on languages is brief and not very precise, and the section on literature contains a good many old, unacceptable ideas and is generally unusable. The same goes for a good part of the section on religion where Islam is not carefully dealt with. In the part called

"The People," Dupree has a section called "Life Cycle," containing observations based on his own experience as an observer of the Afghans, but it is not as systematic as it might be. As is the case elsewhere in the book, the account is remarkably unbalanced for any sort of general treatment, even a "preliminary" one, as he describes this. He covers inheritance, both customary and Islamic, for example, in a paragraph.

For an anthropologist, he has devoted a surprising amount of his book to history. The part on the ancient period begins with a catalog of prehistoric excavations, some of which he has conducted himself, then dwindles away. Much of the section on ancient history is based on some of the older secondary works and contains errors. He takes the whole medieval period in a couple of pages, then lapses into a detailed account of the sensational events of the Afghan conquest of Safavid Iran in the early eighteenth century, repeating what has been said elsewhere. The section on the nineteenth century is for the most part a summary rehash of the British writing on that imperial subject, with "internal events" consigned to sixteen pages of charts and the recommendation that the reader consult a work in Persian by Said Qassim Rishtya. We are not spared Allah striking blows for the Afghans (p. 394), nor can we avoid the "wily Afghans" later on (p. 481).

In "The Present," the account continues to be rambling and discursive, and the unaccountable plunges into detail at the expense of balance continue. There is nothing to supplant or very much supplement such an account as that of Vartan Gregorian's *Emergence of Modern Afghanistan*. It is when the author deals with the period 1949-71, when he was working in the country, that the account has the benefit of his own experience and observations. His American Universities Field Staff reports from the period are well known and frequently cited. This is the aspect of the book that will make it of some value to researchers dealing with recent history. Much of the rest might well have been omitted.

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K. M. DE SILVA, editor-in-chief. *History of Ceylon*. Volume 3, *From the Beginning of the Nineteenth Century to 1948*. (University of Ceylon.) [Peradeniya: University of Ceylon.] N.d. Pp. xiii, 579. \$12.50.

A serious difficulty in carrying on research related to modern Sri Lanka has been the lack of up-to-date sources of historical reference. This solid history substantially improves the situation. And the study is vitalized by the lively talents of young Ceylonese scholars of the University of Sri Lanka.

Part 1 covers the British acquisition of coastal Ceylon (1762–1802) and their conquest of the Kandyan highlands by a strategy similar to that used in cracking the military power of the Scottish clans. Chapters dealing with administrative, economic, social, and religious developments, culminating in the Colebrooke-Cameron Reforms (1832), set the form of the volume.

The focus in part 2 (1832–1910) is on agrarian and export economies and on themes of religion, education, and the civil service and their part in the westernization of the middle and upper classes. Early nationalism is traced through the '48, the Buddhist Revival, the Temperance agitation, and the emergence of bodies such as the Ceylon National Association (1888).

A functional approach is applied in part 3, examining the rise of a new westernized elite to challenge the prestige of traditional Mudaliyars and Kandyan aristocrats. There are surveys of demography, transportation and the judiciary, and the evolution of Sinhala and Tamil language and literature.

Narrative and topical history merge again in part 4: the climax of the plantation era and peasant expansion into the Dry Zone, the growth of Ceylonese self-government (1912–47) discussed in relation to the rise of the Ceylon National Congress, labor unionism, and, subsequently, Marxism. The authors examine the origins of the welfare system, an ever-expanding luxury that subsequent governments rarely dared to reduce for fear of Marxism and rejection at the polls. The final chapter treats the transfer of power from British to Ceylonese in 1947–48.

This work, drawing on original sources, fills in areas not covered in the work of pioneers such as Colvin R. de Silva. The authors view British officials on their merits, without injecting the pique or piety fashionable among many scholars of colonialism. As to Marxism, the authors see its early impact as nationalist and democratic, its ideological force minimized in a largely Buddhist, rural country. The text is frank in discussing the usually hushed question of caste, and there is a fine freedom from communal bias, though a familiar disapproval of Kandyan separatism is discernible. The after-

effects of the 1915 riots seem underestimated. One would have liked more on the interaction between D. S. Senanayake and Sir Baron Jayatilaka in the crucial years preceding independence.

This is an impressive work, distinguished by expertness and sober reflection and full of new material. It could stand as a model for historians beguiled by jargon or bewitched by Delphic utterances from computers. The bibliography includes a comprehensive list of unpublished sources in Sri Lanka, the United Kingdom, and other areas and will supplement H. A. I. Goonetilleke's valuable *Bibliography of Ceylon* (1970). Readers will be grateful for the glossary of important or unusual Sinhala and Tamil terms.

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## UNITED STATES

CUSHING STROUT. *The New Heavens and New Earth: Political Religion in America*. New York: Harper and Row. 1974. Pp. xv, 400. \$12.50.

Although the title of this book relates to the Second Coming of Christ, millennialism does not loom particularly large in it. The expression in the subtitle, "political religion," is used in a very general sense, meaning simply the temporal or social aspects of religion. The book is structured around two themes: first, that religion has been important in American history, and second, that Alexis de Tocqueville was a foreign observer who had some very suggestive insights into the role played by religion—among other things—in the United States. Each topic in the book is centered upon these themes. Thus, when we come to the Revolution we find that "a historical appreciation of religion is necessary in order to understand what happened in '76. Once again the connection is implied in Tocqueville's acute observations" (pp. 50–51).

It is not clear to what readers this book is directed. It is too allusive and, particularly in the beginning, too abstract for the general public. The frequent references to other scholars and their work imply an audience of fellow specialists in American intellectual and religious history; yet surely such people are already familiar with most of what is presented here. General studies that treat broad areas must, of course, depend on previous researches. However, instead of synthesizing and otherwise

using earlier work creatively, Strout all too often simply summarizes existing secondary authorities. His methodology and approach vary according to the historian whose work he is following.

Next to its derivative quality, the book suffers most from its omissions. No rationale is apparent to explain the selection of topics. The treatment of Reformed Judaism can only be called perfunctory. Horace Bushnell, one of the deepest and most influential American thinkers to consider the social implications of religion, is never mentioned. (Neither are Thomas Hooker, Francis Wayland, or Billy Sunday.) Apparently transcendentalism and most of the utopian communities do not fit Strout's definition of "religion"; perhaps this can be justified. But surely such important movements as Know-Nothingism and Christian Science deserve more than the two passing references each receives.

As an overview of the place of religion in the development of American political and social institutions, Strout's work offers little beyond what has already been provided by studies like Ralph Barton Perry's *Puritanism and Democracy* (1944) and William A. Clebsch's *From Sacred to Profane America* (1968). As a general account of religion in the United States, Strout's book is eclipsed by Sydney Ahlstrom's *A Religious History of the American People* (1972). We expect something more than this from a scholar with the ability and reputation of Cushing Strout.

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JAMES W. JONES. *The Shattered Synthesis: New England Puritanism before the Great Awakening*. New Haven: Yale University Press. 1973. Pp. xi, 207. \$8.75.

Professor James Jones of Rutgers University has written biographical sketches of ten seventeenth- and eighteenth-century New England Puritan clergymen. He specifically describes the life and thought of John Norton (1606-63), Giles Fermin (1614-97), Samuel Willard (1640-1707), Cotton Mather (1663-1728), Benjamin Colman (1673-1747), Solomon Stoddard (1643-1729), Lemuel Bryant (1721-54), Ebenezer Gay (1696-1787), Jonathan Mayhew (1720-66), and Charles Chauncy (1705-87). The purpose of this is to illustrate the absence of the coherent "Puritanism" generally presumed (according to the

author) to have been present during the seventeenth century. The Great Awakening was no great explosion of a coherent system but only opened existing disparities to public view. In the beginning, seventeenth-century theologians were able to reconcile such conflicting ideas as the efficacy of human action and the sovereignty of God. But the inner tensions of such a synthesis were uncontrollable, and agreement gradually declined during the course of the century producing competing groups of liberal humanists and evangelical Calvinists. The author emphasizes that each group was a logical product of seventeenth-century Puritan thought—or at least half of it. Eventually, however, each abandoned the emphasis of the earlier generations on harmony and agreement. Each viewed the other with implacable hostility.

As a compendium of biographical data and theological ideas, this is a useful book. Beyond that it suffers from a number of weaknesses. It has been some years since historians of colonial America really believed in monolithic Puritanism. Despite weighty monographs on the subject, there is considerable doubt they ever believed in it. It has been even more decades since historians ceased viewing Puritans as hostile to humanist pursuits. Samuel Eliot Morison's *Intellectual Life of Colonial New England* (1956) might have overstated the case, but the case was surely there. Furthermore, to assert that Puritanism would have inevitably produced Chauncy and Mayhew without the aid of the Enlightenment is rather illogical reasoning bordering on guesswork. Lastly, Jones's acceptance of Alan Heimert's categories of "Liberals" and "Calvinists" perhaps ignores the considerable dissatisfaction with that author's *Religion and the American Mind: From the Great Awakening to the Revolution* (1966).

From a conceptual standpoint there seems little here that is new in an analysis of the basic American religious tensions—that between those stressing the worldly activities of moral men and those emphasizing the workings of the Deity. Almost all writers of the past decade or so have been sufficiently aware of this tension to take it for granted without further elaboration. They simply attempt to show how particular historical personages and events fit the known patterns. In this last respect Jones's book is also useful.

In his search for biographical details Jones has severely limited himself to rather standard sources. For example, Perry Miller's *New England Mind* is used for Stoddard and Mather.



Several get only a reference to the *Dictionary of American Biography*. The analysis of doctrinal developments comes from a study of the individual's sermons.

GEORGE WILLIAM PILCHER  
*University of Colorado,  
Boulder*

RICHARD BEALE DAVIS. *Literature and Society in Early Virginia, 1608-1840*. (Southern Literary Studies.) Baton Rouge: Louisiana State University Press. 1973. Pp. xxiv, 332. \$12.95.

The title of this book would suggest a definitive work on a subject of the first importance, a history of the literature of a part of the country that, in the common view, was too much concerned with agriculture, wealth, law, and politics to devote much attention to the "fine arts." Actually it is rather a collection of miscellaneous essays and addresses of a scholar who has devoted a lifetime to exploring all aspects of the intellectual life of Virginia from the settlement in 1607 to the death of Jefferson in 1826. The chapters are of various kinds, from concern with leftover trivia to swift summaries of general trends, and the book as a whole has no real validity except as filler for the chinks in the more important of Mr. Davis's studies of the culture of colonial and Jeffersonian Virginia and the Eastern Shore of the Chesapeake.

But let us not denigrate it finally for that reason. It is a book that anyone interested in early America should find rewarding in many unexpected ways and that the specialist in this period in American literary history must have, along with the more definitive work of this author.

We can perhaps make a rough division of these chapters into two kinds: first, brief appraisals of relatively little-known gentlemen-scholars, collectors of libraries, and very minor writers of poetry and fiction, and second, essays on general topics. In the first category, we learn about Sandys (and revive the quarrel with Howard Mumford Jones), Fitzhugh, and Gilmer (all subjects of Davis's more comprehensive work), but also the novelists Arthur Blackamore and John Davis and the poets Samuel Davies and James Reid. What we learn will not materially affect the accepted generalization that Virginia's contribution to the American mind was mainly in legal and economic philosophy, classical culture, and revolutionary political theory rather than in belles-lettres, theology, or metaphysics.

Of the essays on general topics, the reap-

praisal of William Byrd II is an exception to the above rule, and such essays as "The Devil in Virginia," "The Gentlest Art" (of letter writing), "The Intellectual Golden Age," "The Valley of Virginia," and "The Jeffersonian Virginia Expatriate" offer fresh insights into familiar history. Especially is this true of the last, which explains the lull in Virginia intellectual and literary life between the death of Jefferson and the great days of Kennedy and Simms as due to the exportation of genius from the local to the national arena. The same was true of the Middle States—especially Pennsylvania—but not, for some unaccountable reason, of New England.

ROBERT E. SPILLER  
*University of Pennsylvania*

PHILIP S. FONER. *Organized Labor and the Black Worker, 1619-1973*. New York: Praeger Publishers. 1974. Pp. xi, 498. \$15.00.

As the main theme in this book Professor Foner adopts the proposition of the young black unionists who organized the League of Revolutionary Workers in the late sixties: "The white labor movement [has] functioned at all times to ensure a special place for the white working man at the expense of the black worker" (p. 422). He offers as evidence a catalog of fifty-eight instances of racism by trade unions and 181 instances of overt trade union discriminations against blacks since 1866. He also closely intermixes a second theme: the necessity of white-black worker unity to achieve victory over the common enemy. On this point he offers 116 instances of cooperation; twenty were successful. As a kind of corollary to these themes he warns that almost every case of strikebreaking by blacks was caused by trade-union discrimination against blacks.

Out of this welter of events trade unions emerge in various states of disgrace. The National Labor Union, the American Railway Union, the railway brotherhoods, and the AFL and its affiliates are without any noticeable redeeming virtue. Some CIO unions—mine-workers, autoworkers, steelworkers, packing-house workers—earn fair to middling but never excellent ratings. The Knights of Labor and the IWW, both of which had little contact with black workers, achieved the best records. Trade union leaders fare no better. Only Michael J. Quill is clean.

On the other hand, the Trade Union Educational League, the Trade Union Unity League,

and the Communist party make enviable records. Even when the American Communists are placing the future of the Soviet Union above the fate of the black workers, as in 1939-41, they remain the undefiled champions of racial equality.

A third theme, which Professor Foner demonstrates with obvious satisfaction, is the growth of black consciousness, which includes everything from the development of the National Colored Labor Union in the 1860s and the activities of the National Negro Congress of the 1930s to the militant movements of blacks since 1960. He describes this recent militancy under the curious title of "the Negro-labor alliance" and defines it as "a combination of union power and soul power" (p. 395). His account covers the whole range of black activities from black-Jew confrontation, Birmingham, Selma, the 1963 March on Washington, the murder of Malcolm X, Watts, the Chicago Freedom Movement, Newark, the Poor People's Campaign, Memphis, the murder of Martin Luther King to the efforts of black workers to break down the union election process and seniority rules in order to advance blacks—a total of forty-nine acts. From this movement he concludes that "the sleeping [black] giant is awakening."

JOSEPH G. RAYBACK  
Temple University

GEORGE A. RAWLYK. *Nova Scotia's Massachusetts: A Study of Massachusetts-Nova Scotia Relations, 1630 to 1784*. Montreal: McGill-Queen's University Press. 1973. Pp. xviii, 298. \$14.00.

American historians have traditionally acquired their knowledge of the early history of Nova Scotia from the books written by J. B. Brebner. In 1972, however, George Rawlyk and Gordon Stewart published *A People Highly Favoured of God*, which offered a religio-psychological alternative to Brebner's belief that geographical isolation forced the neutrality of Nova Scotia's Yankees during the Revolution. Now George Rawlyk offers a comprehensive survey of Massachusetts-Nova Scotia relations that makes further modifications of Brebner's interpretations necessary.

To cover the entire period from 1607-1784 in a book of moderate length means that the author must be judicious in his selection of detail; Professor Rawlyk has succeeded admirably in this task. Fortunately his task of selecting material for emphasis is aided by the basic

fact that the interest of Massachusetts in Nova Scotia, as the author demonstrates conclusively, was fitful and flickering. He is able, thus, to concentrate on those periods, episodes, and figures which show a high level of concern in Massachusetts for the affairs of her northeastern neighbor.

Indeed, the major contribution of this book, aside from the new information it offers, lies precisely in this point: to show that Massachusetts's interest in Nova Scotia was not continuous, universal, pervasive, expansionist, and "imperialist" as previous accounts seem to have suggested. On the contrary, neglect and lack of interest, except on the part of "a relatively small number of fishermen, traders, and governmental officials" (p. xiv), characterized the usual attitude of most people in Massachusetts. Only when the interests and activities of imperial theorists, Boston merchants, land speculators, and the fishermen of Essex County all happened to coincide, usually during a war, was it possible to shake most Massachusetts residents out of their "scorn, ignorance, or apathy" (p. xiv) toward Nova Scotia.

While space does not permit much examination of the substance of the book, one example may be used to illustrate Rawlyk's departure from older interpretations. Where Brebner saw the notorious expulsion of the Acadians as an "inevitable" result of previous Massachusetts interest in Nova Scotia, Rawlyk asserts that it resulted solely from "a military decision made by an inflexible and insecure military man," Charles Lawrence (p. 211). The interpretation of other figures and episodes in the book fits into this pattern.

It must be noted that Professor Rawlyk writes primarily from the point of view of Massachusetts; to understand the internal history of Nova Scotia one must return to Brebner's works, which are supplemented, amplified, and modified, but not replaced, by this book. The notes, which offer a comprehensive guide to the sources used for the book, confirm the author's emphasis on the Massachusetts side of the relationship.

In summary, one may say that this is an important book, based on careful and comprehensive research, written in straightforward, scholarly English, the conclusions of which seem solidly based upon the evidence presented. It seems unlikely that its information and point of view will be outmoded in the foreseeable future.

ARTHUR L. JENSEN  
Westminster College

JAMES G. LYDON. *Pirates, Privateers, and Profits*. Introduction by RICHARD B. MORRIS. Upper Saddle River, N.J.: Gregg Press. 1970. Pp. 303. \$15.00.

This book, less ambitious than its title suggests, is a study of the pirates and privateers who sailed from one port—New York—between 1689 and 1763. Professor Lydon can provide little beyond gossip about the pirates who frequented New York during the 1690s, because these sea robbers left no records, but he has compiled impressive data about the licensed privateers that preyed upon French and Spanish shipping during the wars of 1702–13, 1719–20, 1739–48, and 1756–63. According to this evidence, New York was the privateering capital of North America. Using vice-admiralty court records, newspapers, merchants' papers, and sailors' logs, Lydon has traced the operations of 130 privately managed New York warships; they brought home 684 enemy prizes valued at approximately £2,168,000—a figure that incidentally equals the total value of New York's exports to England from 1697 to 1774. If Lydon is correct, the New York mariners were tremendous fighters, for they took seventy-nine enemy privateers while losing only sixteen ships to the French and Spanish. Another twelve New York privateers were shipwrecked, and more than two thousand seamen were killed, wounded, or captured, but the risk was worthwhile, because generally the merchants who invested in privateering voyages, the lawyers who handled the prize cases, and the sailors who shared in the loot all made excellent profits. Doubtless the man who profited most of all was Vice-Admiralty Judge Lewis Morris, Jr., who issued the privateering licenses and condemned the captured ships and cargoes for twenty-five lucrative years, 1738–62.

Lydon's book has obvious limitations: it is awkwardly written and organized, and it romanticizes the privateers. Yet Lydon solidly demonstrates the large scale of colonial New York privateering. His lists of vessels, prizes, owners, and captains (pp. 271–83) will be particularly valuable to other scholars. Some of his statistics are hard to credit. Out of 109 recorded privateers during the 1739–63 wars, 100 brought home at least one prize ship; are the records of other unsuccessful ventures simply missing? Fuller analysis of the privateering personnel—particularly the merchant investors—is needed in order to demonstrate the role of privateering in the growth of the port of New York. Comparison with other port towns is needed in order to explain why New York was especially

inclined toward privateering. Historians have generally portrayed colonial New York as a commercial backwater compared with Boston or Philadelphia, but Lydon's picture is different: his New Yorkers, eagerly switching from peacetime trade to wartime pillage whenever they calculated that it would pay, seem to presage the rugged entrepreneurship of post-1800 Gotham.

RICHARD S. DUNN  
University of Pennsylvania

LOIS GREEN CARR and DAVID WILLIAM JORDAN. *Maryland's Revolution of Government, 1689–1692*. Ithaca: Cornell University Press. 1974. Pp. xviii, 321. \$15.00.

RONALD HOFFMAN. *A Spirit of Dissension: Economics, Politics, and the Revolution in Maryland*. (Maryland Bicentennial Studies.) Baltimore: Johns Hopkins University Press. 1973. Pp. xiv, 280. \$10.00.

In *Maryland's Revolution of Government, 1689–1692*, Lois Carr and David Jordan pursue two main points. First, they believe that the events from 1689 to 1692 did not constitute a revolution. The Crown's taking over the government of Maryland produced no real change, and no new class of people gained control of the government in Maryland. Second, they believe that the dispute between Baltimore and the Protestants after 1689 had little effect on the lives of most Marylanders, since the local government—the county courts—continued to operate as usual. They emphasize peace, stability, and law and order. They base that emphasis on the absence of records of prosecutions for crimes. Many of the records of the period are missing, however, and therefore what appears at first glance to be negative evidence turns out to be an absence of evidence. In the absence of evidence the authors find it necessary to make extensive use of such words as "perhaps," "possibly," "probably," and "presumably." The result is that their conclusions are uncertain, and the reader remains in doubt about what they have actually shown.

In *A Spirit of Dissension: Economics, Politics, and the Revolution in Maryland*, Ronald Hoffman similarly has two main points. First, in the years before 1775 the attitude of the planters and the merchants of Maryland toward England depended less on what the English Parliament did than on the economic conditions of the province. When business was bad radicals flourished, and when business was good radicalism subsided. Second, after 1774 the ruling

elite of Maryland was as much concerned with the political threat posed by "the multitude" or "the very lowest people" as it was with the conflict with England. The propertied elite established a conservative constitution that would guarantee its continued dominance through property qualifications for voting and holding office, an electoral college for electing the Senate, and the election of the governor by the legislature. In the face of demands that it implement the self-determination it had talked about for propagandistic purposes, and in the face of the conviction of a considerable number of people that American opposition to Great Britain was designed not to protect American liberty and property but rather to enslave the poor people of America, the propertied class retained its power partly by force, partly by creating a large number of new offices with which to purchase the loyalty of those who would fill those offices, and partly by temporarily accepting popular legislation that was to its own disadvantage. The most significant of this legislation was the law by which the legislature in 1777 allowed debtors to pay all of their debts, including those contracted in sterling before 1776, in the depreciated paper money of the province. The effect of the law was to reduce indebtedness and therefore the wealth of the creditors. The law was repealed in 1780.

Hoffman's points are important. Less quoting would have allowed him to make them more quickly and more clearly and would have spared the reader much of the eighteenth-century verbiage. Especially one would like to see a clearer consideration of the speculations of Samuel Chase and his quarrel with the Carrolls. There are also some minor questions of interpretation. But Hoffman has brought order to a very complicated period, and after 200 years of confusion his book is especially appropriate as the first monograph of the Maryland Bicentennial Studies.

C. ASHLEY ELLEFSON  
State University of New York,  
College at Cortland

LARRY E. IVERS. *British Drums on the Southern Frontier: The Military Colonization of Georgia, 1733-1749*. Chapel Hill: University of North Carolina Press. 1974. Pp. xiii, 274. \$12.50.

JOHN FRANCIS McDERMOTT, editor. *The Spanish in the Mississippi Valley, 1762-1804*. Urbana: University of Illinois Press. 1974. Pp. xiii, 421. \$15.00.

Although not intended as companion volumes, these two works have more in common than is obvious at first from their titles. Both are concerned with Spanish policy, one directly and one by inference, and they share a common list of *dramatis personae*: Spanish, French, American, British, and Indian. One views the scene through British and American eyes, while the other scans the picture from a Spanish orientation. It would be only a slight exaggeration to say that these two works present a panorama of Spain's North American experience from 1733 to 1804.

In *British Drums on the Southern Frontier* Mr. Ivers, an Iowa lawyer, undertook a labor of love and did it well. The preface states his purpose to be an emphasis upon "southern frontier politics, Indian diplomacy, and military campaigns from the British colonial viewpoint." He, likewise, admits his fascination with "soldiers' personalities, assigned tasks, efficiency, and life on campaign and in garrison." The motif in this small drama of British-Spanish struggle on the Georgia frontier centers so strongly upon James Oglethorpe that the narrative suffers from lack of focus when he is not on the scene.

The principal contributions of this monograph are twofold. First, by his emphasis upon Oglethorpe, the soldier, Ivers highlights a relatively unknown facet of this complex personality. Second, although the author does little that is new other than state his views on minor points of controversy in the larger picture of the military action on the southern frontier, he does, however, probably achieve a definitive picture, albeit on a small canvas, of the day-by-day lives of the participants—Mounted Rangers, Scouting Boat crews, Indian scouts, British regulars, Highlanders, and colonial militiamen. Here, aided by the pen and ink illustrations of Bill Drath, he re-creates the boredom of frontier garrison life, the excitement of a brief clash of arms, as well as the filth, sickness, and fears of the average soldier. The most striking characteristic of this volume is the exemplary use of clear, informative maps and diagrams. Based upon British and American sources, it is well researched and amply footnoted, with clear and useful bibliographical and index citations. Within its narrow scope, it is an attractive, useful, and entertaining work.

*The Spanish in the Mississippi Valley, 1762-1804* springs from the same source that produced the successful volumes, *The French in the Mississippi Valley* (1965), *The Frontier Re-*

*examined* (1967), and *Frenchmen and French Ways in the Mississippi Valley* (1969). Akin to these in format, it is the product of the fifth of a series of conferences on the Mississippi Valley frontier sponsored by Southern Illinois University, Edwardsville. The 1970 meeting sought "to stimulate interest and to encourage research in the issues and incidents, the practice and personalities of those four decades when the French Mississippi Valley was under the dominance of Spain." The sixteen papers, several in extended form, presented at its sessions comprise this present volume. It is chiefly characterized by variety, as each contributor chose his topic, and a third of them were not professional historians but experts in related fields.

This collection illustrates by its diversity the breadth and depth of the field and should have the desired result of attracting attention and stimulating further study. Here are sound, scholarly, and interesting essays on such topics as land settlement and immigration policies, Indians and Indian relations, Anglo-Spanish rivalries and intrigue, city government and liquor regulation, philanthropy and architecture, and such colorful and controversial personalities as Bryan and Peter Bryan Bruin, Bernardo de Gálvez, Pedro Vial, Philip Nolan, Don Andrés Almonester, and Fernando de Leyba. The major contributions, however, probably lie in three chapters. First would be Professor C. E. O'Neill's forthright discussion of what has been done in the field and the almost unlimited opportunities that remain. A. Otis Hébert's candor concerning the scattered and disorganized nature of Louisiana's collections of Spanish materials is equally valuable, although somewhat discouraging. His essay should be required reading for anyone contemplating work in that state's depositories. Likewise, C. Harvey Gardiner's survey of the Mexican archives and the historiography of the Spanish period will be most welcome to researchers in many fields.

Taken as a whole, this collection is valuable not only for the information and reprinted source material contained in the individual essays, but more especially for the service it performs in providing a ready source of research topics, bibliography, and archival information for students and faculty who, one hopes, will become interested in this badly neglected field of history.

JOHN S. EZELL  
*University of Oklahoma*

PATRICIA GIVENS JOHNSON, *James Patton and the Appalachian Colonists*. [Verona, Va.:] McClure Press. 1973. Pp. viii, 246. \$7.95.

J. LEONARD RAULSTON and JAMES W. LIVINGOOD, *Sequatchie: A Story of the Southern Cumberland*. Knoxville: University of Tennessee Press. 1974. Pp. x, 275. \$9.75.

*James Patton and the Appalachian Colonists* is a biography and a regional history of the New River settlement of Augusta County, Virginia, in the middle of the eighteenth century. *Sequatchie* is the regional history of the southern Cumberland mountain valley of Tennessee from prehistoric days nine thousand years ago to TVA and modern industrialization. Each is a competent and detailed study of little-known segments of American history. Each is based upon original sources and cannot help but be of value to anyone working in the field of the Virginia frontier and the trans-Appalachian West.

*James Patton* is also a good addition to our scant store of information concerning immigrant promotional schemes and the establishment of German and Irish frontier settlements. *Sequatchie* is more of the type of county histories, somewhat less given to critical examination of its materials, somewhat given to boosterism. Twenty-five family histories of nineteenth-century valley settlers are given in the appendix.

*James Patton* lacks style, jolts one with an occasional mistake, such as calling Governor Robert Dinwiddie "Russell" on one page, and sometimes overwhelms the reader with detail. Nevertheless, based in large part upon the Patton Papers in the Draper Manuscripts of the Wisconsin State Historical Society, it is an excellent chronicle of events and the effect of those events upon individuals and their community beyond the mountains in the days of the rapidly developing conflict between France and England for the control of the Ohio valley. Their homes were miles distant over the mountains from Williamsburg, but what happened when the burgesses met, what went on in the governor's palace, what transpired overseas in London and Paris sooner or later interfered with their dreams of land speculation, ran counter to local sentiments, demanded volunteer services without pay, and failed to protect them against savages, and James Patton, "boss" of Augusta County, and his colonists reacted. They were not apathetic.

NATALIA M. BELTING  
*University of Illinois,  
Urbana-Champaign*



JOHN RICHARD ALDEN. *Robert Dinwiddie: Servant of the Crown*. (Williamsburg in America Series, 9.) Williamsburg: Colonial Williamsburg Foundation; distrib. by University Press of Virginia, Charlottesville. 1973. Pp. x, 126. \$5.95.

Although this book is one of a series of popular histories focusing on the role of Williamsburg and Virginia in eighteenth-century America, Professor Alden brings the expertise and mature judgment derived from a long scholarly career to this study of an important Virginia governor. He deftly illustrates how the later career of Robert Dinwiddie was enmeshed with several critical developments: the early thrust over the Alleghenies, the final conflict between the French and British empires in America, and the onset of the political and constitutional crisis that was to rupture the First British Empire. An able man, Dinwiddie made many enemies in the Old Dominion for, as did many other governors, he quickly discovered how difficult it was to satisfy both the Virginians and his superiors in London. The Scot's allegiance was always to Britain, and as a servant of the Crown he could not avoid struggles with the ruling Virginians except by violating his trust. His opponents in Williamsburg, John Robinson, Peyton Randolph, and Landon Carter, were not meek and mild men. Unfortunately for Dinwiddie—since Colonel Thomas Lee who had contested with the Robinson-Randolph clique had died, and in the early 1750s his sons had not yet come to the fore—the governor was faced with a solid phalanx in the Burgesses.

Dinwiddie early raised their ire by insisting on his right to the pistole fee, the charge for affixing the official seal for land patents. His insistence that grants be promptly patented and quitrents promptly paid was critical. In reply to Dinwiddie's demand, "Liberty Property & no Pistole" became a "slogan for outraged land speculators" (p. 27). While hundreds of Virginians applied for patents in accordance with the governor's regulations, and he could rely on a majority of the councilors, the burgesses rejected a compromise that would have established the fee by law. They would "not sanction the charge, legal or illegal, constitutional or unconstitutional" (p. 29). Alden's discussion on this point makes it clear that Virginia legislators did not constitute a monolithic block. The Blairs, the Nelsons, and Corbin sided with the governor, but the Robinsons, the Randolphs, and the Carters, who opposed him, predominated in the lower house. Dinwiddie had to come to an accommodation with the burgesses and their leaders on this and other issues if for no other

reason than to secure money for operations against the French. Alden concludes that Pennsylvanians, Virginians, Marylanders, and North Carolinians, indeed, even Virginians alone possessed ample resources to drive the French and their Indian allies beyond Lake Erie. "But the colonists would not, could not, mobilize a great offensive effort" (p. 65). He seems to agree implicitly with the substance, if not the tone, of Dinwiddie's complaints about the "lethargic stupidity" that seemed to permeate the colonies. Dinwiddie vented his frustration and anger with the burgesses in a letter to a fellow governor: "I assure you I am heartily fatigued and quite weary with the unjust opposition to everything proposed to them for the gen[era]l good" (p. 34). Dinwiddie's experiences during the war induced him to advocate reform of the imperial system. He concluded, and not without reason, according to Alden, that money for defense could not be obtained from the several American legislatures. Yet immediate exigencies led him to make one concession after another to the assembly and its speaker. In the conflict between these concessions and his recommendations to Whitehall lay the seeds of the later imperial crisis.

JACK SOSIN  
*University of Nebraska,  
Lincoln*

FREDERICK W. RATHJEN. *The Texas Panhandle Frontier*. (The M. K. Brown Range Life Series, number 12.) Austin: University of Texas Press. 1973. Pp. xii, 286. \$9.50.

The Panhandle of Texas probably would have been a separate state but for an accident of history, for it has little in common—physically, historically, economically, and socially—with the rest of the state. It, as well as the Panhandle of Oklahoma, developed along entirely different lines from settlements to the east and southeast, as well as from New Mexico, just as it differs in geography from surrounding areas.

Physically the Panhandle is mostly high plains country bounded by the caprock near the 100th meridian (the border between the Panhandle and Oklahoma) as well as to the west near the 103d meridian. Originally this was a vast sea of short grass, a bovine heaven inhabited by buffalo and the Plains tribes whose economy and culture revolved around this beast. Francisco Vázquez de Coronado, the first Spaniard to visit the area, found it the domain of various bands of Apaches, but by the middle of the eighteenth century it was the home of fierce

Comanche warriors who would discourage its settlement for more than a hundred years.

This history of the Panhandle region begins with an appraisal of the physiography of the region, then has chapters on the native inhabitants of the area, the Spanish explorers, and the region at mid-nineteenth century. Next comes an appraisal of the Americans who traversed the Panhandle seeking routes for roads and railroads: Gregg, Abert, and Marcy. Chapters 6 and 7 actually form a single unit, for the actions of the buffalo hunters had both cause and effect in the military conquest of the Indians on the high plains. The final chapter chronicles the settlement of the region by ranchers and farmers.

In this work, a pioneering effort to write the history of this region, the author reflects the heavy influence of Walter Prescott Webb on his thinking. Other than hard facts of history, there is nothing in the volume that is not in *The Great Plains*. Nor is the writing sprightly; rather it reads like a dissertation. And there is no conclusion; the book simply comes to a halt with the turn of the twentieth century—just at the time irrigation was bringing dramatic changes to the region and before mechanization would bring even more change. Despite these limitations, Professor Rathjen has accomplished his stated purpose of detailing the history of this region in a factually correct way.

ODIE B. FAULK  
Oklahoma State University

JOSEPH ALBERT ERNST. *Money and Politics in America, 1755-1775*. Chapel Hill: University of North Carolina Press, for the Institute of Early American History and Culture. 1973. Pp. xix, 403. \$14.95.

It is good to have economic scrutiny more sophisticated than the demands of British manufacturers and merchants on the one hand and their colonial customers on the other. Professor Ernst penetrates the practical pretensions on both sides of the Atlantic. He depicts in their niceties forces that appeared to businessmen and planters in crude shape. The author deals with credit as an often neglected supplement to currency.

The focus of the book is on the Currency Act of 1764, the British incentives, and the colonial reactions to it. He by no means neglects the political flareups, especially in the colonies south of New England, that were the final resort of Americans. He relates rebellious protest to the economic pinches. How much of Ernst's de-

tail, his scenes behind the scenes, will be availed of in future treatments of the American Revolution is questionable. The materials themselves are complicated and not readily reducible to summary statement. Much of this volume consists of slightly modified papers prepared for separate delivery. That makes for repetition and looseness. The final chapter, "The Political Economy of Revolution," might have been used for plain checking of conclusions. Space need not have been given to proposals for further study. The author's advice to his fellow workers in the field might have been offered better in a paper at a scholarly meeting and published in a journal rather than between hard covers.

The reader wonders whether political economy is anything different from what protagonists of conflicting views take it to be. Subtle happenings in the exchanges may be exhibited in slow motion, as it were, but they were perceived by few or none in the scene at the time. The policy of the mother country to restrict the economy of the colonies and the expansion bound to occur in America could not but result in disruption. The economic historian is apt to think otherwise, but it may be argued that economics is politics. Paul Revere was a long ride away from the meticulous graphs of rates of exchange of colonial currencies on sterling. Maybe it was what Revere and his friends thought was the case, not what the graphs reveal, that counted.

BROADUS MITCHELL  
New York City

DIRK HOERDER. *People and Mobs: Crowd Action in Massachusetts during the American Revolution, 1765-1780*. Berlin: Universitätsdruckerei. 1971. Pp. vii, 700.

It was just one generation ago that concerned social scientists stridently voiced their concern over the one-dimensionality of historical writing. Robert Lynd's *Knowledge for What?* appeared in 1939, and the following year the American Historical Association sponsored the publication of *The Cultural Approach to History* in which Caroline Ware, the editor, remarked on the need to "write American history from the bottom up." A world war and the fifties intervened, but in the 1960s a new group of young scholars began to respond to the earlier challenge. For it was a challenge demanding new conceptualization and an accompanying methodology. Often the task was overwhelming: Jesse Lemisch popularized Ware's phrase, but his own writing, while often success-

ful, revealed certain limitations in treating the inarticulate.

So it is somewhat of a surprise that one of the most significant contributions from this approach should appear in a German Ph.D. dissertation, Dirk Hoerder's *People and Mobs*. Heavily influenced by George Rudé, Hoerder traces the history of crowds in Massachusetts from the vantage point of the participants—including women—in the actions, and furnishes us with many new insights. In one section he analyzes the meaning of a word—the “people”—that gives altered significance to eighteenth-century petitions and documents; in a footnote he treats with symbols, noting that “throughout the pre-revolutionary period numbers had strong symbolic value. Now [1776] colors were added. Bostonians hoisted the ‘red flag of defiance.’”

While managing to give some attention to rural riots, his major concern is with urban uprisings. He argues convincingly that mobs in Massachusetts were politically conscious and acted in their own interests against the pre-Revolutionary hierarchical social structure. During the Revolution this ripened into genuine class antagonisms of the poor against the rich, and this feeling was utilized by crowd leaders. The targets selected by the mobs also indicated political awareness: economic riots aimed at things; antiauthoritarian struggles attacked people.

This book is a well-organized and illuminating work. That it is so successful may in part stem from its origins as a German dissertation that relies heavily upon printed sources. Without the usual mania and dependence upon manuscripts his emphasis is on those materials that best relate the experience of the participants in the crowd. With commendable thoroughness he has utilized almost all of the secondary authorities on the subject. Disagreeing with William A. Smith's “Anglo-Colonial Society and the Mob,” he argues that the presence of English troops in Massachusetts, rather than preventing disorders, created much of the discontent. And in his historical survey of riots in Massachusetts, he greatly expands Richard M. Brown's article in *Essays on the American Revolution* (1973), and recasts the intellectual determinist writings of Pauline Maier and her mentor Bernard Bailyn.

It is, then, an excellent dissertation, but it is still a dissertation in need of rewriting with the assistance of a knowledgeable editor. It is a shame that a volume as important as this has been released in such an inaccessible format.

Unless a first-rate university press comes along and puts out a new edition, this book will be a scarce resource for scholars, and its significant insights will not have the impact they deserve.

NORMAN S. COHEN  
Occidental College

HELEN CRIPE. *Thomas Jefferson and Music*. Charlottesville: University Press of Virginia. 1974. Pp. viii, 157. Cloth \$7.50, paper \$3.75.

This book provides the most scholarly study of the relationship between the statesman and the muse yet to appear. More of a handbook than an analysis or narrative, the book's first chapter sets the musical scene, followed by a brief biography in terms of Jefferson's evolving musical interests. Four more succinct chapters delineate the musical education of Jefferson's daughters and granddaughters, his musical instruments and their “mechanical delights,” and the family's music collection. Two appendixes reproduce Jefferson's 1783 music catalog and a listing of his family's music collections; a third discusses his violins. There are several illustrations, a useful glossary of musical terms, and a superb bibliography.

Dr. Cripe has purposely narrowed her focus to the facts; the reader must go elsewhere for their significance for intellectual history. Yet hers was a task worth doing, for Jeffersonian biography has heretofore relied on “that old enemy of the historian, tradition” (p. 25) in describing Jefferson's musical interests and abilities.

The author asserts that music was not Jefferson's “favorite passion,” though he once enthusiastically so referred to it. Cripe furthermore casts doubt on the alleged role Jefferson played in organizing the Marine Band. It is also doubtful that Jefferson, during his later years, played the violin for his grandchildren to dance to. On the other hand, Jefferson, who took up the violin as a young boy, became a talented amateur performer. (“A young gentleman,” Cripe observes, “learned to play a stringed instrument rather than ‘puff out the face in a vulgar fashion with a wind instrument’” [p. 13].) The old saw that “Patrick Henry was the worst fiddler in the colony with the exception of Thomas Jefferson” (p. 89) may be traditional, but, Cripe believes, it is not factual.

Jefferson himself was partly responsible for the myth of the “barbarous” condition of American music in the early nineteenth century. To the contrary, Cripe shows that Jeffer-

son's countrymen supported "a surprisingly large and varied musical culture" (p. 5). At a time of transition from classicism to romanticism—from the harpsichord to the piano—Jefferson took a lifelong delight in every sort of musical entertainment from the vaudeville-like extravaganzas popular in Virginia to the sophisticated offerings of the Paris opera. An amazing array of violins, harpsichords, pianos, and newly invented short-lived instruments came into Jefferson's possession. He had a mechanic's interest in designing better instruments and invented a metronome and a music stand for chamber players. Jefferson also had a keen ear—he tuned his own keyboard instruments. His musicianship helped him to win the musical Martha Wayles Skelton away from his rivals, and he carefully followed his progeny's musical education. Whether making music with German officers of Burgoyne's Convention Army ensconced near Charlottesville or attending a concert of the great violinist Viotti in Paris, Jefferson made music a constant life companion. If his musical interest was less intense than his interest, for example, in gardening, architecture, or literature, music nevertheless remained his lifetime "resource . . . against ennui." It was "the companion which will sweeten many hours," and above all, concludes the author, it was his "delightful recreation" (p. 93).

THOMAS WENDEL  
California State University,  
San José

JAMES H. O'DONNELL, III. *Southern Indians in the American Revolution*. [Knoxville:] University of Tennessee Press. 1973. Pp. xii, 171. \$8.50.

With so much scholarly attention now being devoted to the American Indian and the American Revolution, it is not surprising that several studies focusing on the Indian role in that struggle have recently appeared. The book considered here is not one of the better examples.

O'Donnell's narrative deals with the involvement of the major southern tribes—Cherokee, Creek, Choctaw, and Chickasaw—in the Revolutionary War. While both the British and the rebels hoped to exploit Indians for military advantage, Indian objectives were mainly to protect their lands and their trade. Accordingly, the Indian role in the conflict was basically defensive. Indian military involvement began with the Cherokees' struggle to defend their lands in 1776 and ended with Indian efforts to defend not only their own settlements, but

the last British strongholds. Since the major threat to the fundamental interests of the tribes came from the rebels, the British had most of the advantages in competing for Indian favor. The rebel governments were more than willing to settle for Indian neutrality, and they were generally successful in dividing and neutralizing the tribes.

O'Donnell's study has some value as a convenient, year-by-year summary of the war as it involved the southern Indians. It is nonetheless a disappointment. Despite considerable research effort, no new interpretations are offered, and little new information. The brevity of O'Donnell's account, moreover, leads to unfortunate abridgments and omissions. Little attempt is made to relate the struggle in the southern backcountry to that on the northern frontiers or to the Revolutionary War as a whole. The consistent and effective support of the rebel cause by the Catawbas is not mentioned; nor is Loyalist cooperation against the Cherokees in 1776; nor is Chickasaw activity against Clark's men at Fort Jefferson in 1780.

Perhaps most serious, for an author who views his subject as largely a "clash between two civilizations" (p. 16), O'Donnell's treatment of Indians and Indian cultures is surprisingly superficial. Major works dealing with southern Indians during this period are ignored (for example, Gearing and Woodward on the Cherokees, Gibson on the Chickasaws, Brown and Hudson on the Catawbas). Little attention is devoted to Indian political and military practices, the role, and even the identity, of Indian leaders is hardly noticed, and there is no sustained effort to describe and explain the fatal Indian factionalism.

JOHN T. JURICEK  
Emory University

RUDOLPH M. BELL. *Party and Faction in American Politics: The House of Representatives, 1789-1801*. (Contributions in American History, number 32.) Westport, Conn.: Greenwood Press. 1973. Pp. xiii, 311. \$13.95.

Rudolph M. Bell's monograph is the first contribution to the extensive body of literature on politics in the 1790s that fails to cite a single contemporary document. The full burden of his analysis rests on a tabulation of roll call votes in the lower house of Congress derived from the *Annals of Congress* and the 1826 edition of the House journals. Computer processing identified the clusters of issues and delegates that produce the most cohesive voting patterns.

Bell's aim is "to demonstrate that these mathematically optimum combinations make sense within a historical framework" (p. 270).

Since he chose not to use any evidence except the roll call record, Bell's study has an extremely narrow focus and is highly abstract. He is concerned only with how congressmen voted; he is not concerned with why they voted as they did, what they thought, how they bargained with each other, how they reacted to actions by the Senate or the executive branch, what they heard from their constituents, or how parties and factions operated in their home states. The voting factions he describes consist of men who, Bell admits, "did not knowingly plan strategy against each other and . . . never thought of themselves as participants in the faction in which the present analysis places them. . . . [T]heir votes fell into patterns that they neither saw nor acted upon" (p. 154). Bell invites us to picture the situation in Congress during the 1790s as "two growing circles, labeled respectively 'Federalist' and 'Republican,' floating in a House along with a variety of smaller and generally contracting circles, the factions" (p. 183).

Historians who work with a wider variety of evidence will probably argue with Bell's conclusions. He asserts that issue-oriented factions effectively solved all of the nation's problems by 1785 and thus made it possible for the new office-oriented parties to function. One might as logically assert the opposite, that the rise of party machinery forced members of factions to compromise their principles and acquiesce in policy decisions they might otherwise have continued to resist.

Few historians today would deny that computer-assisted research has made extremely valuable contributions to our knowledge of early America. But a computer program is not a substitute for research in primary sources.

LINDA GRANT DE PAUW

*George Washington University*

EDMUND BERKELEY and DOROTHY SMITH BERKELEY. *John Beckley: Zealous Partisan in a Nation Divided*. (Memoirs of the American Philosophical Society, volume 100.) Philadelphia: the Society. 1973. Pp. xiii, 312. \$6.00.

This is the first book-length biography of John Beckley. Its thesis is that Beckley deserves, with Madison and Monroe, continuing recognition for having been the primary organizer of the early Republican party. Prevented by poverty

and illness from running for major public office, Beckley was overshadowed by the well-known and wealthy Virginians who controlled the party.

Born in England, Beckley came to America as an indentured servant to become a clerk for John Clayton, a prominent Virginia botanist. From that lowly position he gradually rose to become Clerk of the House of Delegates of Virginia, and he later served as mayor of Richmond for several terms. His primary claim to fame, however, is based on his position as Clerk of the National House of Representatives throughout most of the period from 1789 to 1805. It was in this position that he became a zealous partisan of the Republican cause, indiscriminately condemning all Federalists including Washington. The authors demonstrate convincingly, to my mind, that Beckley wrote many of the anonymous partisan attacks on Federalists that appeared in pamphlet form during this period. He played an important role as a party and campaign organizer in the pivotal state of Pennsylvania during Jefferson's successful campaign for vice president and, eventually, for president in 1800.

The book contains few surprises. The authors describe Beckley's responsibility for revealing the well-known and tragic story of adultery between Alexander Hamilton and Mrs. Reynolds. Unfortunately, the tale is told with such minute attention to detail that it loses much of its poignancy. Interestingly, however, the authors have convincingly cleared Edmund Randolph of the charge of complicity with the French minister. It was this celebrated affair that precipitated Randolph's dismissal from the cabinet and finally resulted in Washington's endorsement of the Jay Treaty for ratification.

In the final analysis the book is disappointing. It is largely a chronicle of Beckley's life and activities. As a consequence we learn as much about his poor health, his land speculation, and his poverty as we do about his role as a party organizer. One must wade through mountains of often unrelated trivia with considerable puzzlement why a person of secondary importance should be the subject of such laborious and massive research. Only in the final half-dozen pages do the authors make clear the important role that Beckley played in the success of the early Republican party. Nowhere is it evident to me what motivated Beckley to be the "zealous partisan."

In short, he remains the mystery man, a characterization that the authors had hoped to dispel. Even so, we are indebted to them for a



book that will someday lead to the definitive work that will pierce the mystery.

GENE D. LEWIS  
*University of Cincinnati*

ARTHUR SCULLY, JR. *James Dakin, Architect: His Career in New York and the South*. Baton Rouge: Louisiana State University Press. 1973. Pp. xiv, 209. \$15.00.

"There was a Mr. Dakin, a young man of genius, who had been a carpenter and had studied architecture in Town's office. . . ." So wrote the architect James Gallier of James Harrison Dakin, the designer who gave this Irishman his first job in the United States. This was in 1832, at the very time when Alexander Jackson Davis was beginning his spectacularly successful career in New York. To date Dakin has remained a legend, and this not surprising, for he failed to make the contacts that ensured the fame of Davis. Furthermore, he was a man of scant education and never enjoyed, as did Davis, the chance of taking long walks with Herman Melville.

Bringing Dakin back to life presents a challenge. Even though he was the architect of the Gothic capitol of Louisiana, the building that caused so much amusement to Mark Twain, he left behind almost no correspondence on which to hang his accomplishments, which means that his biographer, Arthur Scully, Jr., has been deprived of the type of documentation that makes for an easily digested book.

But Mr. Scully is not easily discouraged, and readers of this life will have the privilege of learning probably all that may be learned about his hero. We do not know what was the occupation of Dakin's father; we only know that the architect-to-be was a native of Dutchess County, New York, and got his chance in 1829 when at twenty-three he joined the firm founded by Ithiel Town and Alexander Jackson Davis. Six years later he was off for New Orleans. It was in the South that he was to make his mark.

Scully reminds us, and the evidence he brings forth is convincing, that Dakin, not Gideon Shryock, was the architect of the Bank of Louisville. But he refuses, and this is all to his credit, to say an unkind word about historians who have attributed this monument to Shryock.

Dakin was to go bankrupt in 1841. This was after he and his brother Charles had designed the Verandah Hotel in New Orleans, which went up in flames in 1855. Still another hotel

by James Dakin's hand was the Gayoso in Memphis, like the Verandah burned to the ground. The architect died in 1852 after having trained Henry Howard, the creator of Belle Grove, perhaps the greatest of all antebellum houses in the Deep South.

The many color plates are rewarding, and the author's attempt to give James Dakin and his brother Charles their exact due is a model of scholarship. The author's guesses are also well informed. Who knows? James Dakin may have been the man who designed the now-vanished Gothic Afton Villa at Saint Francisville.

WAYNE ANDREWS  
*Wayne State University*

ROBERT E. SHALHOPE. *Sterling Price: Portrait of a Southerner*. Columbia: University of Missouri Press. 1971. Pp. xii, 311. \$12.00.

Born into a respectable family that had slipped to the lower fringes of the Southside Virginia establishment, Sterling Price struggled mightily to join the South's slaveholding elite. He finally made it in Missouri as a tobacco planter, merchant, and general entrepreneur, and he tenaciously retained his hard-won prominence. A conservative Jacksonian Democrat, he worked his way through the jumble of Missouri politics as a part-time protégé of Thomas Hart Benton, who helped him get a commission in the Mexican War.

Concluding his mediocre military performance with a reckless but successful offensive into enemy country, General Price came home a hero, and from 1853 through 1856 he was governor of Missouri. A hard-money conservative, he only grudgingly allowed limited state subsidies for popular railroads, and his strong pro-slavery policy ended his unnatural alliance with Benton. Price was convinced that his new prominence and prosperity depended upon slavery, but he was also a staunch Unionist. When the Union ruptured, he wavered, but, when fighting actually started in Missouri, his Southern heritage prevailed.

Confederate General Price commanded everything from divisions to small armies at Wilson's Creek, Lexington, Pea Ridge, Iuka, Corinth, Helena, Jenkins' Ferry, Pilot Knob, Westport, and other battles and skirmishes. Essentially an amateur, he was a mediocre tactician and a poor administrator who neglected logistics, discipline, and other vital aspects of modern warfare. He was pompous, impulsive, and secretive, and he often quarreled with superiors up to and includ-

ing President Davis. Yet the tall, distinguished-looking Missourian was a fearless fighter who inspired devotion and loyalty in his ragged citizen-soldiers, and he was so committed to the cause that he sojourned in Mexico after the war before returning home to die in 1867.

Robert E. Shalhope, associate professor of history at the University of Oklahoma, has examined Price's life thoroughly. By painstaking research he has re-created the essential Price, devoting more than half of the text to the antebellum period, which Albert Castel's 1968 biography treats only very sketchily. Shalhope probably exaggerates both the difference between the "gentility" of Virginia and the "strident opportunism" of Missouri and the contradiction between Price's "republican ideals" and "entrepreneurial wishes," but still he skillfully analyzes a proud, status-conscious pseudoaristocrat facing troubled times. Inadequate maps weaken the battle descriptions, and some ponderous prose remains from the original dissertation, but, overall, this scholarly monograph is a mature study of a flawed hero of the Confederacy.

F. N. BONEY

*University of Georgia*

DENNIS CLARK. *The Irish in Philadelphia: Ten Generations of Urban Experience*. Philadelphia: Temple University Press. 1973. Pp. xvii, 246. \$10.00.

The book investigates the adjustment to city life of Irish immigrants, enduring the stress of urbanization and industrialization. The steady transformation is presented as a social process against a background of the history of the Irish land problem, stretching over three centuries, or ten generations as the subtitle suggests. Specifically, the study probes the adaptation of the Irish in the context of mid-nineteenth-century Philadelphia when these rural people fresh from the fields and villages of Ireland, with no tradition of town government, abruptly became residents of the East Coast's major industrial city. The author focuses on two topics that he treats simultaneously. The famine generation's plight details the struggle to survive and to prosper. Philadelphia's characteristics, as they emerge in the framework of Irish immigration, reflect the city's large population in relation to the number of newcomers and its capacity to employ them.

The investigation accentuates the immigrants' divestment of their cultural heritage and their attraction to a bustling city that dulled the haunting memories of their decaying homeland.

It describes the social and economic conditions that enabled the immigrants to find low-cost housing and jobs in textile factories. Stress is given to the unique political setup that provided room for activities in both parties and thus led to a dispersal of Irish political strength, in contrast to Boston and New York where the Irish dominated the Democratic urban machines. The discussion also emphasizes the role of the well-built structure of Catholic Church and school that added social and ethnic functions to the religious and educational institutions and separated the Irish as a group from the general life of the city. The book emphasizes the human dimensions of emigration as well as the continuity of the transformation and in the final section links the attainment of status in the United States to the emigrants' critical role in the fight for an Irish national state.

The study furnishes fresh insight into the complex relationship between the Irish and Philadelphia. Occasional references to New York, Boston, and Newburyport provide glimpses of a broader context of the Irish experience. The investigation demonstrates the newcomers' stamina in coming to grips with the urban conditions and the city's role in facilitating the adjustment. It enhances our understanding of the role of the boss-contractor and the Catholic clergy in making possible the flowering of an ethnic subculture. This intriguing assessment of the Irish as an urban problem discloses a weakness of the book not mentioned among the shortcomings discussed in the preface (p. xv). The treatment of the Irish emphasizes the necessity for understanding people and their needs, but the discussion of Philadelphia, the other major component of the study, does not penetrate the life and the milieu of the mass of residents.

GUNTHER BARTH

*University of California,  
Berkeley*

CARL N. DEGLER. *The Other South: Southern Dissenters in the Nineteenth Century*. New York: Harper and Row. 1974. Pp. 392. \$10.00.

Continuity distinguished the course of Southern history in the nineteenth century, according to Mr. Degler, in opposition to the point of view of another able historian of the South. He traces this continuity through the political dissenters—the antislavery men becoming opponents of secession, then Unionists, and after

the Civil War, scalawags. The thesis is a good one thus far, but I cannot see an important connection with the Populists, his last group of dissenters. His study is well documented by personal papers, unpublished Ph.D. theses, and articles from historical journals, notably the state historical journals, but newspapers of the period are neglected. One of the unpublished theses that he frequently cites is that of Patricia Hickling, "Slavery in Virginia, 1831-1861," whose conclusion that a considerable proportion of the antislavery men were Whigs he accepts (incidentally he misspells her name, adopting the Southern habit of speech of dropping final *g*'s). The author's analysis of the antislavery men contains very little that has not been told before, but he tells the story in vigorous, lucid prose. He divides the antislavery men into two classes, those motivated by concern for the slaves and those, the much larger group, who opposed slavery because of what the institution did to the whites—an organizational device that obscures the mixed motives of most of the antislavery men. He notes that the antislavery men, with the notable exception of Helper, were normally slaveholders from the border Southern states, that they often had been educated or lived in the North, and that many of them were Whigs, but he failed to observe that most of them came from the professional class.

The author's discussion of the years from 1860 to 1877 gives the best insight into the troubled South that I have recently read. An interesting part of the book is his account of the antisecessionists, in which he holds that the majority of the people, even of the lower South, were opposed to secession, a controversial view to which I subscribe after a recent study. Another valuable part of his volume in his discussion of the scalawags and the motives that led a considerable number of Southerners after the Civil War to become Republicans, presenting a wide spectrum of opinions. The account given of Mahone and the Readjusters is especially able, portraying the decline of an essentially liberal movement because of its cooperation with the Negroes. For a similar reason the Populists failed, although they had remarkable success in North Carolina for a short while. One of the sad aspects of Southern history, which the author vividly portrays, is the success of anti-Negro propaganda, such as that following the Danville and Wilmington riots, in turning the people away from progressive movements. Throughout this admirable study Degler writes from a modern liberal point of

view, showing a great empathy for the dissenters and for the Negro.

CLEMENT EATON  
*University of Kentucky*

DANIEL AARON. *The Unwritten War: American Writers and the Civil War*. (The Impact of the Civil War: The Civil War Centennial Commission.) New York: Alfred A. Knopf. 1973. Pp. xix, 385, xiv. \$12.50.

This is a study of the views of a highly selective group of American writers toward the Civil War. One may cavil about the narrowness of the selectivity; fewer than three hundred names out of thousands are mentioned, and many of these receive only a nod of recognition. But one can hardly quarrel with the selections themselves. They include the most eminent Northern and Southern literary figures of the latter half of the nineteenth century and of the twentieth century, from Ralph Waldo Emerson and William Gilmore Simms to Edmund Wilson and William Faulkner.

The author develops his themes by separating the writers into Northern and Southern groups, then subdividing these into sets according to chronology, participation in or proximity to the war, literary mode, or outlook. For example, the Northern poet Walt Whitman and the Southern novelist John Esten Cooke represent men of letters who experienced the war in one form or another; Henry Adams, Henry James, William Dean Howells, and Mark Twain make up a set of malingerers who chose to stay as far from the scene of action as possible; George Templeton Strong, David Hunter Strother, and Mary Boykin Chesnut are diarists of unusual perceptiveness and literary skill; Stephen Crane and Harold Frederic, having grown up after the war but being steeped in its lore, are able to blend concrete episodes with abstract situations and characters to write classic stories about it. Attitudes run the spectrum from intense patriotism and enthusiasm, either Union or Confederate, to deep revulsion. The writers who get the most sympathetic treatment, regardless of region or period, are those who see the war philosophically and who "sound its tragic import." Among these are Melville, Whitman, and Faulkner.

Yet the author is not satisfied with the war writings of any of these figures. He concludes that none has been able to re-create the "real" war of death, disease, vermin, hunger, misery, heartache, crime, vice, and cowardice as well as heroism—Everyman's war. This is the sense

in which, to him, it remains an "unwritten" war. Novelists may bridle at this criticism, but historians ought to be gratified to hear that "as yet no novel or poem has disclosed the common soldier so vividly as the historian Bell Wiley does in his collective portraits of Johnny Reb and Billy Yank" (p. xviii).

This work is well written and well informed, a worthy addition to Civil War literature.

CHARLES P. ROLAND  
University of Kentucky

ADRIAN COOK. *The Armies of the Streets: The New York City Draft Riots of 1863*. [Lexington:] University Press of Kentucky, 1974. Pp. x, 323. \$14.50.

Civil disorders are inherently interesting, and few are more so than the New York City draft riots of 1863. Mob unrest, racial antipathy, draft resistance, and the fears of a war-torn society combined in the three-day holocaust that came to symbolize nineteenth-century racial and urban unrest. Finally, in *The Armies of the Streets*, these riots receive a book-length scholarly analysis.

Mr. Cook brings new evidence and questions to the topic. Previous accounts narrated in detail the horror and the violence of these days but reflected stereotypic views of mobs and/or ethnic groups and suffered from incomplete documentation. Cook's discovery of the records of the indictments and grand jury dismissals and his careful use of all relevant county and military archives allow him to correct the hurried and inaccurate contemporary accounts and to produce a narrative that is more complete if less dramatic. Most significantly Cook demonstrates that the death toll of the riots was from 105 to 119 rather than from 800 to 1500, and that the general level of violence was lower than earlier estimates.

This book goes beyond the stereotype of the mob. Cook's identification of 352 rioters reveals that this crowd was much like those that George Rudé has studied. It was not comprised of riffraff. Of the 168 with identifiable occupations, one-third were skilled laborers or higher, and all were employed. Participants rioted within or near their own neighborhoods, and few had criminal records. Nor did outsiders manipulate this mob. Spontaneity characterized its action. Widespread opposition to the draft and to the Civil War and deep race hatred ignited the powder—a deep-seated resentment of living conditions and of an authority that allowed poverty and wealth to exist in close

proximity. Inadequate mechanisms for social control created the necessary conditions for this and the sixteen other major civil disturbances in the city between 1834 and 1874.

Yet Cook's study falls short in some serious ways. He analyzes the crowd so briefly that his conclusions are unclear or unconvincing. While he labels this crowd "a mob of the industrial age," his evidence suggests the preindustrial mob of Rudé and Hobsbawm. His failure to employ the manuscript census returns and city directories prevented occupational and residential identification for over fifty per cent of his sample. The dynamics of racial violence remain unexplained. Despite his argument, fear of economic competition is a more convincing explanation than "intense racial prejudice." That whites attacked black neighbors or co-workers is important, but what does it mean? The absence of a detailed map of lower Manhattan makes it difficult to identify streets and neighborhoods and follow the mobs' action.

Cook's exhaustive use of official records to supplement newspaper and contemporary accounts provides a definitively accurate narrative. His estimate of the death toll and his list of casualties and rioters will stand unrevised. Yet only with a more careful analysis of the social characteristics of the rioters will the fuller meaning of the draft riots emerge.

WILBERT H. AHERN  
University of Minnesota—  
Morris

JOSEPH J. MATHEWS. *George W. Smalley: Forty Years a Foreign Correspondent*. Chapel Hill: University of North Carolina Press, 1973. Pp. x, 229. \$10.95.

At a time when the massive, one-thousand-page biography has become as expected and commonplace as the three-volume novel was in nineteenth-century England and Russia, it is refreshing to come upon a biographical study that attempts to encapsulate its subject in less than two hundred pages. Joseph J. Mathews has not only attempted but has succeeded within this limited scope in giving the reader the essential facts in the life of George Washburn Smalley. The author portrays the man with the same economy of strokes that Harry Furniss used in drawing the cartoon of Smalley that now adorns the title page and the dust jacket of this book. Mathews has also succeeded in placing Smalley within the larger historical context of his time, as battlefield correspondent in the Civil War, as the New York *Tribune's*

foreign correspondent in London from 1866 to 1895, and finally, as the London *Times's* foreign correspondent in New York and Washington from 1895 to 1905.

For four decades Smalley was America's best-known foreign correspondent, reporting on wars and politics, the theater, and literature with a self-confident finality and an Olympian judgment that made his initialed by-line a byword on two continents. His fame both as a writer and as an organizer of America's first overseas press bureau was well deserved. Had Louis Napoleon shown the same organization and enterprise in his encounter with the Prussians in 1870 as Smalley did in reporting it, the Battle of Sedan might have had a different conclusion.

Moving with a kind of glacial inevitability, as Mathews reveals, from being a "radical in the sixties, to moderate Liberal in the seventies to Tory in the eighties and after," Smalley never questioned his own consistency or the absolute correctness of his "impartial and objective view of the news." Firmly ensconced for three decades within the tight little intellectual circle that dominated London society, he regarded himself as America's real ambassador to Britain, who must put up with ineptitude and cultural crudities of such impossible but happily temporary official diplomatic appointees as Reverdy Johnson and Robert Schenck, whom the White House persisted in sending to the Court of St. James's. Smalley ultimately became more interested in guiding than in reporting the news, but his efforts as behind-the-scenes mediator in the Venezuelan crisis of 1895 clearly demonstrated that his talents were better utilized with the reporter's pad than with the diplomat's portfolio. Dismissed by Reid after thirty years of service to the *Tribune* for being too pro-British, Smalley was to have the distinction of being dismissed by the London *Times* ten years later for being too pro-American (and pro-Russian) in reporting the Russo-Japanese War. It is doubtful if Smalley ever understood or appreciated the singular role he played in journalistic history by being an unwitting double agent.

This is an honest biography written with style, succinctness, and a candor that is both genuinely appreciative and properly critical of its subject. It is a distinguished addition to the other volumes on the history of American journalism that have been published by the University of North Carolina Press.

JOSEPH F. WALL  
Grinnell College

ROBERT RYAL MILLER. *Arms across the Border: United States Aid to Juárez during the French Intervention in Mexico.* (Transactions of the American Philosophical Society, New Series, volume 63, part 6.) Philadelphia: the Society, 1973. Pp. 68. \$3.00.

A primary catalyst to United States aid for Mexico during the French intervention in the 1860s was the secret agent. The activities of these men were clandestine, their hopes and plans those of desperate dreamers. Utilizing original Spanish, American, and French sources, Professor Robert Miller offers an excellent, personalized account of the efforts of these heretofore neglected agents. In so doing he has written the first work focusing on the military support received by the Juaristas from the United States.

With the possible exception of Matías Romero, the republican ambassador in Washington, these agents were self-seeking altruists. Each believed in Juárez, detested the Frenchman's foot that polluted Mexican soil, and was willing to sacrifice purse and limb—each for a price.

In exchange for their efforts the American agents were promised commissions in the Mexican army, tracts of land, mining sites, railroad rights, and substantial sums of money. They were given their promised commissions. Yet only one received payment for his services and this after a thirty-year struggle. The other rewards went the way of *mañana*.

Miller provides ample evidence that the Mexican agents neglected no avenue in their efforts to secure American arms and men. Each attempted the thankless task of floating Mexican bonds at a time when their native currency was valued on a par with Confederate dollars. They organized Mexican clubs and Monroe Doctrine societies. They applied direct and indirect political pressure. And at every turn their intrigue was met with counterintrigue by French agents. Their loaded ships were often impounded before they hoisted anchor. Those fortunate enough to sail often foundered en route or arrived in Mexico only to be sacked by rebellious Mexicans.

Yet despite their frustrations and failures, these agents were successful in forwarding some desperately needed arms and men. That the material aid they acquired for the Juaristas was important is undeniable. That it proved a "major contribution to the republican war effort" and that "the militant resurgence of the Mexican republicans would have been impossible without the aid secured by secret agents



in the United States" are, however, highly debatable conclusions. Yet it is upon such conclusions that good history thrives. Robert Miller has written good history.

RICHARD CRAIG  
Kent State University

DANIEL J. BOORSTIN. *The Americans: The Democratic Experience*. New York: Random House. 1973. Pp. xiv, 717. \$10.00.

This last volume of Boorstin's trilogy describes how the technological innovations of the last one hundred years have transformed the daily experience of ordinary Americans. Essentially, Boorstin claims, revolutions in applied science and the enterprising spirit of American "go-getters" made the American experience democratic: we all came to share the same experiences. Boorstin is more or less convincing in various parts of the book, but he is at his best in examining the effects of technology on changing patterns of consumption, ownership, and morality; he has striking things to say about western beef raising, the sewing machine, and the private bath. On these and other subjects germane to the social history of the years since 1865 there is an extraordinary bibliography that is the most sustained and persuasive eighty pages in the volume. Despite his ill-concealed contempt for the liberal professoriate and the "rigid categories" of academic history, Boorstin has provided dissertation topics for scores of aspiring doctorates.

The central theme of *The Democratic Experience* is the breakdown of old communities and the growth of new ones. Boorstin defines a community as a collectivity sharing certain behavior patterns, and this allows him to speak of pervasive "consumption communities," for example, all those people who bought from the Montgomery Ward catalog, or even those who wore a size forty suit. Commentators have already ridiculed this attempt to defend the anonymity of modern America by usurping a word that really designates a better and more spiritually pure entity, and it is true that Boorstin often manages to associate the honorific connotations of the word "community" to his *gemütlichlos* collectivities. But Boorstin is also wiser than his critics: by concentrating on a behavioristic criterion of community and ostensibly avoiding the debate on the moral overtones of the word, he is able to argue that his diffuse and dispersed communities join people "by countless gossamer webs knitting together the trivia of their lives." The com-

munity of Lucky Strike smokers is slight compared to that of the Visible Saints of the Massachusetts Bay Colony, but twentieth-century Americans are members of countless, more casual communities than the few intense ones shared by seventeenth-century New Englanders. Spatio-temporal propinquity, his argument goes, determined common behavior patterns in the past; today forces irrelevant to geography determine these patterns. The decline in old-style, face-to-face communities is a function of the decline of space and time as a determinant of community. As community membership ceased to depend on geographic location and more and more people counted as members of given communities, the experience of each person lost its uniqueness. In short, American experience became democratic, for it is community membership that determines experience's contours.

Boorstin is not content simply to note that spatial and temporal bounds no longer define communities. He argues that in modern America space and time are overcome. Window glass and air conditioning erase the distinction between outdoors and indoors; television's instant replay arrests the fleeting moment; nationwide franchising makes consuming a repeatable experience. There may be something in all this, but Boorstin mistakenly thinks his argument has metaphysical significance. He really seems to believe that time has become "fungible," that it is no longer possible to distinguish appearance and reality. On this issue, however, Kant is still right and Boorstin wrong. Men still die—they don't escape time; and there is a difference between watching the 1968 convention on TV, even watching it over and over again, and being clobbered by Mayor Daley's policemen.

Whatever the source of Boorstin's ideas on the fundamental form of modern American experience, they lead him into a paradox. In terms of his earlier work, the triumph over space and time that the new communities entail means that the "givenness" of experience has disappeared for Americans; nothing is given, all is synthetic, artificial. At the same time that Boorstin recounts the triumph of mind over nature, however, he laments the persistent American subordination of ideals to facts. In areas as diverse as sexual behavior, grammatical usage, and diplomacy, Americans unfortunately accept what "is" as a warrant for what "ought to be." On the one hand, this misguided respect for the givenness of experience seduces Americans into confusing the descriptive with the

prescriptive; on the other, Americans have so mastered the world of space and time that the given has ceased to exist.

*The Democratic Experience* is riven with this contradiction. The hallmark of American democracy is the supremacy of the given (apparently an evil) while the democratization of America is the tale of a vanishing given (apparently a good). The source of this confusion may in part be the ambiguous nature of American history, but it is surely in part Boorstin's own divided sensibility, which celebrates the democratization of America but loathes its result—American democracy.

This conflict may account for some of the weaknesses of the book. Boorstin's understanding of anticommunist foreign policy is embarrassing in its naiveté. He characterizes the United States as pursuing Samaritan diplomacy, perhaps bungling but always good willed, if often antidemocratic. He actually writes that Americans got "a new sense of the community of man" after using the atomic bomb. While the nondemocrats in the State Department get a positive appraisal, Boorstin writes with almost physical distaste of their opponents, the American left and youthful radicals; the extreme democrats evoke his visceral dislike.

While Boorstin excels in describing the surface consequences of technology on Americans, he is unable to confront the effect of advancing industrial capitalism on the human psyche. The adjectives that describe the American experience are always those that bespeak a measurer at work—he can quantify experience as thick or thin, deep or shallow; the quality of life in the United States does not concern Boorstin.

Minimal regard for convention demands that reviewers evaluate the work of major historians as must reading. *The Democratic Experience* is must reading, but the book is additionally worth pondering. Boorstin is the latest in a line of gifted American thinkers—from Thomas Jefferson to Josiah Royce—with a deep theoretical concern for the nature of American community and with a tortured conscience about the meaning of the American past. While admiring his gifts, I cannot help but be disturbed by his bitterness about the present and his almost willful misunderstanding of it: like many historians, Boorstin's work troubles me as much as this nation's history troubles him.

BRUCE KUKLICK  
University of Pennsylvania

ESTELLE F. FEINSTEIN. *Stamford in the Gilded Age: The Political Life of a Connecticut Town,*

1868–1893. Stamford, Conn.: Stamford Historical Society. 1973. Pp. xi. 319. \$6.95.

Up until 1893, the year in which this study ends, Stamford's 16,000 inhabitants were governed by a town-and-borough system. Interference from the state capitol at Hartford was minimal, and the Gilded Age in Stamford produced neither the bossism nor corruption that plagued other municipalities during this era. The traditional town meeting provided an aspect of order and continuity within the community (although, according to the author, it was far from a democratic institution), and the appearance of the professional politician had not yet threatened the established ruling order. Nevertheless Stamford's government became increasingly outmoded in view of the need for expanding local services. The charter of 1893, in which the city was formed—albeit with the retention of the town government and town meeting system—was a direct outgrowth of the awareness of the inadequacy of the administrative machinery to provide greater flexibility for the governing of the community.

Students of urban history will be grateful to Professor Feinstein for her excellent research in tracing the political life of Stamford. The selection of this type of community, however, raises certain questions. Stamford was not a "modern industrial city"—the term used by John A. Garraty in his foreword. It appears to have been remarkably resistant to change. Stamford not only escaped bossism, but it seemingly was spared other problems as well. Immigrants moved to the Irish sector of the town, yet no single or fixed pattern of neighborhood hostility existed within the community. Other cities in this period were experiencing a change in personnel in terms of the kinds of officials elected to public office. Stamford was not, and there was little difference in the socioeconomic background of the men elected to office between 1868 and 1893.

Despite the author's general reluctance to criticize the community, she takes note of the "insular view" of the local ruling faction and its concept of an "essentially static society." As Feinstein points out, Stamford, like the nineteenth-century island community described by Robert Wiebe in *The Search for Order: 1877–1920* (1968), viewed innovation with skepticism, even as it realized that change was inevitable. Had this theme been the focal point of the book, a more precise picture would have been drawn. The Gilded Age appears not to have had a major impact upon Stamford, although

this useful book is a valuable source of information for documenting the pattern that did emerge.

DIANA KLEBANOW  
University of Bridgeport

JOHN E. BODNAR, edited and with an introduction by. *The Ethnic Experience in Pennsylvania*. Lewisburg: Bucknell University Press. 1973. Pp. 330. \$12.00.

This book is a collection of essays that, to put it as charitably as possible, would have been better left uncollected. This is not to say that the major flaw of the book, uneven quality, is at all uncommon in works of this sort. In this particular instance, however, the malaise is especially acute since the worst of these offerings probably should never have seen the light of day under any circumstances.

Nevertheless it should not be inferred from the preceding remarks that this book is totally devoid of redeeming social importance. On the contrary, several of the authors have shed considerable light on the forces that impelled the new immigrants to leave their homelands as well as on their adjustment to life in the United States. In this category is an excellent article by Caroline Golab focusing both on the circumstances behind the flight of Poles from their homeland as well as on the factors that account for their pattern of dispersal once they crossed the Atlantic. Her conclusion that immigrants tended to settle where jobs existed for which they were qualified is hardly startling, but nevertheless is worth stating, since most accounts stress kinship ties or points of origin as determinants of where immigrants went once they disembarked in an American port. This point is also developed in Richard N. Juliani's interesting study of the Italian community of Philadelphia.

Another very significant theme is developed separately in studies of Italian, Croatian, Serbian, and Ukrainian immigrants. The scrutiny of the experiences of these people both in their homelands and in Pennsylvania discloses that they arrived in the United States almost devoid of feelings of national identity, usually owing to their submergence in ethnically diverse nation-states dominated by others. Upon arrival in the United States each of these immigrant groups tended to cluster together geographically, and under these circumstances each developed feelings of national identification far stronger than those brought from Europe. Juliani's work on the Italian immigrants of Philadelphia illus-

trates this point perfectly, leading the author to conclude that those he studied "while being 'Americanized,' became 'Italian' also, before their former countrymen in Italy did" (p. 250).

In spite of these and other strengths, one is nonetheless still hard put to justify this publication since it lacks the coherence and unity necessary to make it useful. To have achieved the necessary coherence and unity would in turn have required greater selectivity in determining which essays were included as well as the addition of a perceptive introductory analysis designed to place the disparate stories presented by the contributors into a more comprehensible framework. Since neither of these approaches were followed, those concerned with ethnic history would probably have been just as well served if the authors had submitted their work individually to refereed journals.

RICHARD L. EHRLICH  
Eleutherian Mills—Hagley Foundation

DAVID M. KATZMAN. *Before the Ghetto: Black Detroit in the Nineteenth Century*. (Blacks in the New World.) Urbana: University of Illinois Press. 1973. Pp. xii, 254. \$10.00.

This book emphasizes the period between 1870 and 1900, when the number of blacks in Detroit increased from 2,235 to 4,111 and the white population grew from 77,000 to 281,000. After an opening chapter covering the years prior to 1870, the book deals topically with residential patterns, caste, work, class, and politics.

Katzman argues that despite the abolition of slavery in Michigan in 1837, the enfranchisement of blacks in 1870, and the repeal of the ban on interracial marriage in 1883, most blacks in Detroit remained locked into a caste system that was perpetuated into the twentieth century. A tragic sameness remained for blacks in Detroit, no matter if they numbered 2,800 as in 1880 or 120,000 as in 1930. The reasons for this sameness are found in the caste system, imposed by the dominant white society, and the class structure, imposed by upper- and middle-class blacks. The blacks at the top, refusing to accept the ethnic model, adopted the value system of white America. This system stressed individualism and rejected the ideal of cooperation except when it assisted personal gain.

Overall the book is a major contribution to the growing body of literature dealing with racial and ethnic minorities in urban America. While we might question the emphasis the author gives to caste as the explanation for the condition of blacks in Detroit, we should not

allow this debate to blur the many worthwhile descriptions and insights found in the book. Segregation patterns were evident in Detroit with the clustering of blacks in what might seem integrated neighborhoods. After World War I these clusters formed the basis for the black ghetto. The lack of decent maps in the book, however, makes it difficult to follow these residential patterns.

The descriptions and analyses of black institutions and the class structure are excellent. The upper class, which was not part of the caste system, interacted with the white community on all levels, particularly in politics. This class looked down on the lower class and generally blocked the mobility of the middle class. Out-migration and the end of political influence caused the upper class virtually to disappear by 1900. The middle class, made up of businessmen and skilled workers, lived in an almost totally segregated world. Many were newcomers from the South. They were torn by a desire to be like the elite and by their economic investment in a segregated community. This was the class that replaced the upper class after 1900 and, as Allan H. Spear writes in *Black Chicago: The Making of a Negro Ghetto, 1890-1920* (1967), institutionalized the ghetto.

The lower class, made up of both the deserving poor and the "disreputable poor," felt the full impact of discrimination. Katzman uses Oscar Lewis's work to show that the lower class was caught in a subculture of poverty. Here the author is at his weakest. There has been all too much controversy among social scientists over the issue of black culture for the author to deal with it so briefly. Throughout the book Katzman emphasizes caste; yet he devotes much more space to upper-class blacks, who were outside the caste system, than to lower-class blacks. He also fails to discuss the influence that color had in determining the black social structure.

Katzman concludes that the twentieth-century ghetto was built on the foundation of the nineteenth-century community. But he gives us only pieces of twentieth-century history. The book would have been more valuable if he had carried his history through World War I.

RICHARD J. MEISTER  
University of Michigan—  
Flint

STEPHEN GOTTSCHALK. *The Emergence of Christian Science in American Religious Life*. Berkeley and Los Angeles: University of California Press, 1973. Pp. xxix, 305. \$10.00.

This is an important addition to the recent efforts of scholars like Robert Peel and Raymond Cunningham to analyze Christian Science within its full historical and cultural context. The author's greatest contribution is to delineate more completely than has anyone else the intellectual substance of Christian Science. Chapters 2 and 3 of his book are especially significant in this regard. In the first of these chapters Gottschalk argues the philosophical and theological distinctiveness of Christian Science vis-à-vis both orthodox and liberal Protestantism of the late nineteenth and twentieth centuries. In the latter of these chapters the writer considers the close interconnections between Christian Science and the welter of "mind-cure" movements that flourished in the Gilded Age, and yet also argues convincingly for the uniqueness of Christian Science within this larger intellectual and religious tradition. The succeeding sections of the book discuss such subjects as the personal role of Mary Baker Eddy in the movement from the 1880s to the time of her death in 1910, the practice of healing in relation to the movement, and the social characteristics and attitudes of those attracted to Christian Science.

Although Gottschalk is sympathetic in his evaluation of Christian Science, he is hampered, as have been all other students of this movement, by the restrictions placed on the use of the official church archives. Gottschalk bases his account chiefly on printed sources—the many editions of *Science and Health with Key to the Scriptures*, and the numerous periodicals, pamphlets, and books published by Scientists in the formative years. His evaluation of the career of Mrs. Eddy possesses a blandness that seems to belie the earlier turbulent and controversial historiography. One wonders how this portion of the author's analysis might have differed given unrestricted access to the full manuscript record.

The author could also have developed more fully the implications of some of his findings. For example, he notes briefly (p. 244) the key role women besides Mrs. Eddy played in the Christian Science Church, yet he never relates this fact systematically to the changing role of middle-class women in a rapidly industrializing society. A more detailed examination of the parallels in practices, as well as ideas, between Christian Science and evangelical Protestantism (pp. 192-93 offer hints of the possibilities of such analysis) could help to explain more clearly why Christian Science attracted so many "mainline Protestants" to its ranks. And there

is an all-too-brief section analyzing the social characteristics of Christian Scientists.

In summary, as an intellectual history of the formative years of the Christian Science movement (stressing especially the years 1880–1910) this is solid fare. As an attempt, however, to offer an explanation of why Christian Science emerged as an important indigenous religious movement at the particular time and place and in the peculiar form it did, Gottschalk's book is only partially persuasive. The historiography of Christian Science is advanced by, yet still must move beyond, this work.

JAMES FINDLAY

*University of Rhode Island*

R. ALTON LEE. *A History of Regulatory Taxation*. [Lexington:] University Press of Kentucky. 1973. Pp. xi, 228. \$15.25.

This well-written, concise volume surveys the American experience with regulatory taxation from its late nineteenth-century beginnings to the present. After an introductory chapter that surveys the conflicting views about the extent of the taxing power and how the use of regulatory taxation became one of the two bases of the national police power, the author surveys in chronological order the attempts to use regulatory taxation to control oleomargarine production, impure food and dairy products, commodity futures, phosphorus matches, narcotics, child labor, agricultural and coal production, firearms, and gambling. In so doing he presents succinct explanations of the conditions leading to the passage of the taxes, the debates over them in Congress, the Supreme Court's judgments on their constitutionality, and assess-economic, social, and political interests that ments of their effectiveness.

The strength of Professor Lee's study lies in its clear portrayal of the variety of conflicting were either aided or offended by such taxes. His discussions of the differing motives behind the various taxes and the means whereby they were translated into both practical and constitutional arguments in the congressional debates are fascinating. They remind us that American constitutional development can best be understood through a comprehensive approach rather than one centering on narrow juristic analysis.

Despite the segmented presentation caused by the organization of the volume, the author's treatment of the technical aspects of the major court decisions on regulatory taxation is pre-

cise, and his skill in placing each in its proper doctrinal and historical context greatly enhances the value of the work. The contrasting rationales presented by the Supreme Court as it veered back and forth in split decisions between a permissive view of the scope of the taxing power as in *McCray v. U.S.* in 1904 and the sharply restrictive view enunciated in *Hammer v. Dagenhart* in 1918 are clearly illustrated. Though the author documents the Supreme Court's willingness to treat national police-power measures based on the commerce power with more tolerance than those based on the taxing power, he does not explain the reasons for this behavior. The precedents restricting the use of the commerce power for regulatory purposes were no less numerous than those applicable to the taxing power.

One can find little argument with Lee's conclusions that the use of regulatory taxation has come "full cycle—from first hesitant use by Congress in 1886 to doubt about its efficacy as a means of control almost a century later," and that the Supreme Court's "interpretation of regulatory taxes offers an interesting illustration of the trend in national constitutional growth." However, the author, while covering the details of the Supreme Court's treatment of regulatory taxation, fails to analyze the implications of the inherent subjectivity in the judicial process. Had he done so, he might not have been so quick to forecast the impending interment of regulatory taxation—even in light of the Supreme Court's recent acceptance of Fifth-Amendment inspired protection against self-incrimination in registration requirements for gambling taxes.

This one qualification notwithstanding, Lee has written a work that greatly clarifies a complex part of our constitutional development.

JAMES C. DURAM

*Wichita State University*

BENJAMIN FRANKLIN COOLING. *Benjamin Franklin Tracy: Father of the Modern American Fighting Navy*. [Hamden, Conn.]: Archon Books. 1973. Pp. xvi, 211. \$10.00.

One of the hardest components of American constitutional faith is the belief in civilian control of the military. It is a principle almost never disputed by either the public or military men. The content of the latter is understandable. More frequently than not, their interests have been well served by their civilian superiors.

Take, for instance, Benjamin Tracy, secre-



tary of the navy under Benjamin Harrison (1889-93). A lawyer, New York judge, and friend of the notorious Boss Platt, he took over the navy department innocent of any naval experience or learning. Yet his administration modernized and strengthened the navy and launched it on the trajectory to greatness. His achievement was on a par with Elihu Root's better-known reform of the army a decade later.

Mr. Cooling's book covers the whole of Tracy's life (1830-1915) and thus becomes the first biography of the man. But it is by no means the first book to examine Tracy's reign at the navy department. In their pioneer studies, Margaret and Harold Sprout and John A. S. Grenville and George Berkeley Young called attention to the seminal importance of Tracy's annual report of 1889 and the subsequent legislation of 1890. And Walter R. Herrick, Jr. devoted the bulk of his *American Naval Revolution* to depicting Tracy as the man "directly responsible" for the establishment of a battle fleet able to operate offensively. Covering much the same ground, Cooling arrives at a different and somewhat ambivalent view of Tracy's contribution. He prefers to see developments as evolutionary, situated harmonically in the political and technological context of the 1880s and 1890s. In that context, Tracy emerges as a talented manager who transformed the original thoughts of others into action. We may conclude that he was, if not the "father" in the book's title, at least the chairman of the paternity committee.

This book will be welcomed by specialists in naval history as a handy source of detail. For example, it contains an ample discussion of Tracy's negotiations with the producers of armor plate. Most readers, however, will regret the relative lack of critical analysis.

WILTON B. FOWLER  
*University of Washington*

JOHN M. HARRISON and HARRY H. STEIN, editors. *Muckraking: Past, Present, and Future*. Foreword by IRVING DILLIARD. University Park: Pennsylvania State University Press. 1973. Pp. vi, 165. \$10.00.

This stimulating collection of eight essays grew from a conference held at Pennsylvania State University in May 1970. While the editors report the participants' views on the future of muckraking and Carey McWilliams shows continuity in the muckraking tradition, the volume's point of reference is the golden age of the movement, ca. 1902-12.

Most contributors agree with Louis Filler that "what is at stake" in any assessment of muckraking "is our estimate of the American middle class." Filler believes that the first muckraking movement was vital because, in contrast with later elitist generations, it understood and spoke to the values, fears, consciences, and interests of the middle class. Precisely this preoccupation with the fears of white readers, argues Robert C. Bannister, Jr., made the popular muckraking magazines more timid and racist than their more elitist contemporaries. Like their readers, the muckrakers saw lynching and peonage not as problems of race but as parts of their larger concern with lawlessness and corruption. David M. Chalmers argues that that concern with lawlessness, which they defined as a consequence of the destruction of older values and institutions by the sudden growth of large-scale units of economic power, blinded them to the inner workings of the "justice-delivery system" when they infrequently looked at the courts, police, and prisons.

John G. Cawelti and Jay Martin describe the current impact of the muckraking tradition. Cawelti maintains that authors like Jacqueline Susann, Harold Robbins, Allen Drury, and Arthur Hailey reflect the same popular hunger for behind-the-scenes exposures of elites that had nourished the first muckraking movement, while showing how they use exposure to reinforce prevailing values and institutions. Martin likewise argues that the muckrakers' "aesthetic of exposure" dominates contemporary popular culture, but he claims that our practitioners have shed the first muckrakers' utopianism and defined man himself as the evil and thus generated an "aesthetic of apocalypse" to replace earlier reformism.

The contributors lacked space to explore at any depth the connections between readers and muckraking. Why has the American middle class wanted muckraking at some times and places and not others? If muckraking is uniquely American, as the editors assert—and one wonders how and why the exposures of William Cobbett and the popular press of nineteenth-century Britain or *Der Spiegel* today differ from the American phenomenon—then what explains this uniqueness? How and why does the public want to believe Watergate exposures now? Why have readers or listeners created the movement described in this book?

DAVID P. THELEN  
*University of Missouri—  
Columbia*

ROY V. SCOTT and J. G. SHOALMIRE. *The Public Career of Cully A. Cobb: A Study in Agricultural Leadership*. Jackson: University and College Press of Mississippi. 1973. Pp. viii, 287. \$9.95.

The life of Cully Cobb illustrates the familiar truism in American life of the poor boy who made good. Conscious of this, the authors open their narrative with Cobb's reflections in 1936 as he returned to Mississippi State University to speak to a conference of rural ministers.

As the son of a country preacher, Cobb had pulled himself up by his own bootstraps in a tradition-bound society. At the university, then the Mississippi Agricultural and Mechanical College, he became known for his industry, ambition, and integrity. During his senior year the legislature passed the agricultural high school law. Cobb became principal of the school at Buena Vista.

His influence increased when in 1910 he took the task of directing the Mississippi boys' corn club work in connection with the Department of Agriculture's cooperative demonstration program. He worked with the rural schools, through county superintendents and teachers, to organize clubs and used various means to publicize his work, enlist recruits, and obtain supplemental support for prizes and for trips. The Smith-Lever Act of 1914 provided increased financial resources. He soon established clubs for other crops, pig clubs, baby beef clubs and other livestock clubs, and community agricultural clubs.

In 1919 Cobb became editor of the *Southern Ruralist*. He considered farm journalism one of the greatest educational institutions for the advancement of farming and the development of agricultural thought. Essentially Cobb's paper was a periodical for the small white farmers of the Southeast. In 1930 the *Ruralist* was merged with the *Progressive Farmer*.

In addition to his work with editing the paper Cobb had helped organize the American Agricultural Editors Association. He was in constant demand as a speaker. Asserting that the farmer wanted the government to treat agriculture as it treated other groups, he became a militant advocate of the McNary-Haugen bill in the 1920s.

In 1933 he was appointed chief of the Cotton Production section of the Agricultural Adjustment Administration in Washington. His earlier experience afforded him contact with businessmen, farmers, and extension personnel. He urged that the new program be carried out by the extension service network. Although the

Agricultural Adjustment Administration was a New Deal agency, it was involved in an internal and departmental struggle between groups known as liberals and conservatives. Cobb resented what he considered interference in the operation of his program, as the liberals sought to effect social as well as economic improvement. This complex problem is discussed solely from Cobb's point of view.

On September 1, 1937, Cully Cobb left the AAA to go to the Ruralist Press, Inc., a commercial printing corporation. Here he stayed until he sold it on June 24, 1971.

Scott and Shoalmire have pointed out that Cobb's accomplishments reflect his background, training, and environment. He learned to accept what he could not change and to seek practical workable solutions within the system of which he was a part. The study has added another facet to Scott's writings on agricultural education, farmers' institutes, extension activities, and their leaders, writings that have made him an authority in the field.

VIVIAN WISER

*U.S. Department of Agriculture,  
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LARRY D. HILL. *Emissaries to a Revolution: Woodrow Wilson's Executive Agents in Mexico*. Baton Rouge: Louisiana State University Press. 1973. Pp. xi, 394. \$12.95.

From April 1913 to October 1915, Woodrow Wilson designated eleven agents to report on the course of the Mexican Revolution. During these thirty-one months the president's men superseded diplomatic and consular officers as the actual United States representatives. As a group, despite their prominence in Wilson's policy, these presidential officials have never been studied. To complete accounts of the United States's response to the Revolution, Larry D. Hill recounts the activities of these emissaries and evaluates their influence on the formulation of Wilson's diplomacy.

Hill writes about the adventures of the agents in a lucid, often witty, style. Although most of them lacked experience in diplomacy or knowledge of Spanish or both, they did permit Wilson to use Democrats in pursuing his foreign policy without playing the spoilsman by wholesale removal of the American representatives in Mexico. Without ignoring the other seven appointees, Hill properly gives greatest attention to William B. Hale, muckraking journalist and Wilson's campaign biographer; John Lind,

progressive Democrat; George Carothers, consular agent of dubious reputation; and Leon Canova, excellent newspaperman and political opportunist. The book is based securely on research in both Mexican and American manuscripts and archives. Hill's extensive investigation enables him to relate confidently the agents' difficulties in gathering intelligence while trying to boost the reputation of his favorite rebel with President Wilson.

Although skillfully reporting the activities of these emissaries, Hill is not altogether convincing in his efforts to demonstrate that the agents had a major part in the development of policies. Certainly they provided much of the information on which Wilson made decisions, but he appointed them because they had perceptions similar to his own. Their suggestions may have increased his policy alternatives, but they advised little the president could not adduce from his own political views. Although the author briefly refers to the moralistic idealism of the diplomacy, he avoids the interpretive controversy concerning the assumptions that directed Wilson's responses to the Mexican Revolution. A test case is found in Hubert L. Hall's recommendations that the United States recognize the importance of Zapata's insurgency. The administration dismissed his advice and attempted to reach Hall to dismiss him. The proposition that the agents had much influence on policy formulation remains contention. It is just as well; it is enough that the book is a well-written narrative of Wilson's emissaries in Mexico.

WILLIAM H. BEEZLEY  
North Carolina State University

CHARLES B. BURDICK. *Ralph H. Lutz and the Hoover Institution*. (Hoover Institution Publications 131.) Stanford: Hoover Institution Press, Stanford University, 1974. Pp. xv, 185. \$9.50.

Well-endowed research libraries do not come into existence through wishful thinking, or even the philanthropy of their benefactors, but primarily through the quiet determination and painstaking selection and acquisition by librarians and collectors. If anyone doubts this truism, his doubts should be laid to rest after reading the story of the Hoover Institution on War, Revolution, and Peace, and the role played by its first director, Ralph H. Lutz.

Professionally Lutz was not a librarian, but a historian with a special interest in European history. His undergraduate work had been done

at Stanford University, where one of his teachers was Ephraim D. Adams. At the close of World War I in 1918, Adams conceived the idea of assembling on the Stanford campus a collection of material dealing with the war and its consequences. Adams succeeded in interesting Herbert Hoover in financing the project. Also, Adams persuaded Lutz, who was then stationed in Germany as an officer in the American army, to undertake the work of collecting material. Later Lutz himself was appointed to the Stanford faculty.

Ralph Lutz was a quiet man who possessed great determination and perseverance. Having undertaken to initiate and build up the Hoover War Collection (as it was first named), he made this his mission in life for the next twenty years. His work involved frequent trips to Europe and brought him in contact with collectors of all nationalities, not all of whom were sympathetic with his objective. He was obliged to subordinate his personal views and convictions on many occasions. In 1925 Lutz was appointed chairman of the Hoover Library. He continued as the director and the guiding spirit of the library until he was forced by illness to retire in 1943. Even after that he continued an active interest in the library until his death in 1968.

Lutz's problems did not end with the acquisition of materials from abroad. As the head of a specialized research institution, he objected to any subordination to the teaching-oriented Stanford University Library. This brought him into conflict with two university librarians, who looked upon a library primarily as a repository for published records, while Lutz emphasized ephemera such as leaflets, posters, proclamations, private papers, and photographs. Their disagreements could be resolved only by providing the Hoover Library with its own building in 1941. Today the Hoover Institution on War, Revolution, and Peace with its collection of over one million volumes stands as a monument to the quiet determination of Ralph Lutz, as well as the philanthropy of Herbert Hoover.

THOMAS S. HARDING  
Washburn University of Topeka

THOMAS E. VADNEY. *The Wayward Liberal: A Political Biography of Donald Richberg*. [Lexington:] University Press of Kentucky, 1970. Pp. xi, 223. \$9.75.

This study is chiefly concerned with its subject's public life; only passing attention is given to his relations with his parents, his

troubled first marriage, his financial difficulties, and his lifelong quest for social acceptance. Thus focused on the outer rather than the inner man, it dwells at length on Richberg's career in Chicago reform politics, his work for the Progressive party, his prominence as a labor lawyer during the 1920s, his role in the early New Deal, and his subsequent legal and political interests.

Sometime during the early 1930s—while NRA general counsel, director of the Industrial Emergency Committee, and so-called assistant president to Franklin Roosevelt—Richberg was transformed from a crusading liberal into an equally crusading reactionary. The onetime antagonist of the trusts found himself in the front ranks of their legal skirmishes, numbering among his clients the very institutions whose power he had once deemed inimical to the public interest. Later, he tried to help find legal ways of circumventing the Supreme Court's desegregation decision in the famous *Brown* case of 1954. At his death in 1960, then, Richberg appeared to have completed the left to right odyssey undertaken by so many twentieth-century American liberals and radicals.

But for Vadney, all was not as it seemed; what really changed was liberalism, not Richberg. He remained true to his principles throughout his life. And so, perhaps, he did. But Vadney's argument is extremely weak. He fails to indicate clearly what liberalism was at the turn of the century and what it has since become. Nor does he clearly spell out what Richberg's political principles were. His own evidence can be used to muddle his interpretation. For example, the early Richberg defended the poor and the unorganized against powerful corporations. The later Richberg defended the powerful and the affluent against the federal government. The early Richberg had an abiding faith in lawyers as guardians of the public interest; the later Richberg, on Vadney's account, appears to have had an abiding faith in the lawyer's role as defender of special private interests. Whether these contrasts can be incorporated into Vadney's argument is uncertain, especially since Vadney himself fails to make the effort.

Hence this volume contains some interesting, if not very surprising, information about a "Wayward Liberal" encapsulated in an interpretation that leaves one unconvinced and unsatisfied.

THOMAS A. KRUEGER  
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SANDER A. DIAMOND. *The Nazi Movement in the United States, 1924-1941*. Ithaca: Cornell University Press. 1974. Pp. 380. \$15.00.

Scholars of American anti-Semitism, immigration, German-American relations in the 1930s, and fringe group dynamics will especially profit from Sander Diamond's extensive research. Of particular value is the information from captured German documents and the papers of the German-American Bund—information that must have cost heavily in patient work. Despite several name changes from 1924 to 1941, the American Nazi movement was essentially a continuous development that, for convenience, may be called the Bund.

Misled by the myth that race prevailed over ties of citizenship or birthplace, Nazi officials in the early 1930s held high hopes for awakening and unifying millions of Americans of German descent into a force to influence the United States government. The Nazi party (NSDAP) supported the Bund movement as one instrument for that awakening. But by 1935, the Bund had made almost no impact on German-Americans. Further, ventilation of its endless infighting had damaged the Bund. And the McCormack-Dickstein hearings of 1934, stressing close Bund connections with the Nazis, had associated it in the public mind with German intrigue. Also, Berlin realized that Bund activities were hampering German-American diplomatic relations. Consequently, by late 1935 the NSDAP regarded the Bund as a handicap to its American program and decided to break with it.

Ironically, directly after the NSDAP jettisoned it, the Bund entered its strongest period (1936-39), a time when vigorous leadership by Fritz Kuhn built membership to about twenty-five thousand. But, as Nazi Germany's repression and aggression rapidly alienated Americans in the late 1930s, Kuhn found his more visible Bund increasingly feared as a potential Nazi fifth column. In December 1939 officials in New York strangled the Bund by shipping Kuhn to Sing Sing for larceny of Bund funds.

Diamond's valuable discussion of Bund membership indicates that only ten per cent were American-born. Most Bundists were German nationals who had immigrated since 1920. They were young, tended to be skilled workers, and were very hard hit by Depression unemployment. Diamond asserts that hallucinations about Jewish power in America kept them in fear of a Jewish-led pogrom. A primary motivation, then, for joining the Bund was self-protection.

In a book as extensive as this, it is surprising

that little appears concerning important Bund centers outside metropolitan New York. Also, the chapter on the movement in the 1920s is too limited. Substitution of a note on archives for a complete bibliography detracts from the book's usefulness. And lack of a final summary or conclusion is especially unfortunate in view of the organizational problems in this book. While Diamond's prose is clear, repetitiveness and poor organization make for extremely difficult reading. Central topics appear for a couple of pages, give way to detailed digressions, reappear, submerge before some other central topic, then resurface. The reader must assume the burden of unraveling and reorganizing.

DAVID S. WYMAN

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Amherst*

SUSAN ESTABROOK KENNEDY. *The Banking Crisis of 1933*. [Lexington:] University Press of Kentucky. 1973. Pp. x, 270. \$13.25.

This carefully researched and clearly written study focuses on the events leading to Franklin D. Roosevelt's decision to suspend all of the nation's banks on March 6, 1933, and on the various considerations that shaped the administration's policies, not only those dealing with the many immediate problems of determining which institutions were to be allowed to reopen but also those treating with the larger questions of long-term reform of the financial system. No other single work provides so comprehensive a review of that crisis; none rests upon so thorough an examination of the available manuscripts and specialized secondary literature. The study begins with a review of the major weaknesses in the nation's financial structure during the 1920s, describes how the stock market collapse of October 1929 aggravated the inadequacies of the banking system, and explains why President Herbert Hoover's policies failed to prevent the massive bank suspensions that occurred between January 1, 1930, and March 15, 1933. During that period, 5,790 banks with deposits of nearly \$3.5 billion closed their doors. Mrs. Kennedy's account of this dismal chapter in the history of American finance includes, as it should, the impact of international events on the mounting crisis, most notably the tremors that shook the banking system when foreign depositors started withdrawing gold from the United States. Some of the largest amounts were taken out during the six weeks following the middle of September 1931. Nor does the author ignore the grave damage done to public

confidence in the banking system that resulted from the Senate subcommittee's disclosure of widespread misconduct on the part of some of the nation's leading and most respected financiers.

Some of Mrs. Kennedy's findings will be familiar to the specialist. But even in those instances where her study relies on earlier scholarship, it almost always provides some fresh and interesting facts, many of them taken from previously neglected or little-used manuscript collections. This is especially true of the author's richly detailed analysis of banking problems during the interregnum, which she argues were made worse by the fact that the nation "suffered a fatal lack of leadership," a view amply documented in the chapter dealing with the various attempts, all of them unsuccessful, to salvage Michigan's shaky banking structure. Hoover's indecisiveness contrasted sharply with Roosevelt's bold, swift actions. The study ends with an analysis of the Emergency Banking Act, the procedures established to reopen sound banks, and the reforms written into the Banking Act of June 1933. Mrs. Kennedy's appraisal of these events is informed, judicious, and balanced.

VINCENT P. CAROSSO

*New York University*

PHILIP J. FUNIGIELLO. *Toward a National Power Policy: The New Deal and the Electric Utility Industry, 1933-1941*. [Pittsburgh:] University of Pittsburgh Press. 1973. Pp. xvii, 296. \$12.95.

As one of the most dramatic aspects of the New Deal, the power fight of the 1930s has long since become the subject of a voluminous literature. What seems new in recent years is the appearance of scholarly studies relatively free from the moralizing and emotionalism that characterized much of the older work. One thinks, in particular, of Thomas McCraw's recent *TVA and the Power Fight: 1933-1939* (1971). And added to McCraw now must be this generally excellent monograph by Philip Funigiello.

Indeed the two works should be read together. For Funigiello's contribution lies primarily in illuminating aspects of the subject with which McCraw did not deal. He has not studied extensively and has little new to say about the conflicts over the TVA, the power pooling idea, or the Seven Sisters' proposal. But he offers a thoroughly researched and well-conceptualized study of the origins, passage, and effects of the Public Utility Holding Com-



pany Act plus shorter studies of rural electrification, the Bonneville Power Administration, and the National Defense Power Committee. On these matters, moreover, his contributions are substantial. He provides not only a wealth of new and carefully documented detail, some of it correcting accepted accounts of the "death sentence" provision and the functioning of the Rural Electrification Administration, but also an enlightening analysis of the ideological, bureaucratic, personal, and interest-group conflicts that made policy formation such a complex process. And throughout he shows how the search for a coherent national policy, one that presumably would have produced better results than the "piecemeal, *ad hoc* arrangements" actually adopted, was consistently thwarted by internal debates between anti-trusters, planners, and cooperationists, inherited structures of private, local, and federal power, and the penchant of Roosevelt and other New Dealers for expedient arrangements and administration by conflict.

If Funigiello's work has flaws, they lie chiefly in spots where he tends to allow legislative and bureaucratic minutiae to overwhelm his larger design and themes. The reader quickly tires, for example, of the extended descriptions of congressional hearings and interchanges of memorandums. He wishes repeatedly that the author would make his point and move on, and I felt that the section on the Bonneville Project could have benefited greatly from judicious cutting, focusing, and condensation. Such faults, however, are minor. On the whole, students of the New Deal are indebted to Funigiello for a solid and important contribution to understanding the course that power policy took in the 1930s.

ELLIS W. HAWLEY  
*University of Iowa*

GEOFFREY S. SMITH. *To Save a Nation: American Countersubversives, the New Deal, and the Coming of World War II*. New York: Basic Books. 1973. Pp. xii, 244. \$10.00.

CEDRIC BELFRAGE. *The American Inquisition, 1945-1960*. Indianapolis: Bobbs-Merrill Company. 1973. Pp. xvii, 316. \$8.95.

STEFAN KANFER. *A Journal of the Plague Years*. New York: Atheneum. 1973. Pp. xi, 306. \$7.95.

According to the authors of these three books, Americans were intolerant of the extreme Right in the 1930s (Smith), and of the Left in the 1940s and 1950s (Belfrage and Kanfer), and were wrong both times because the dangers ap-

prehended in the three decades were mythical, or largely so. Father Coughlin and his Christian Front, William Pelley and his Silver Shirts, and Fritz Kuhn and his German-American Bund were what Professor Smith calls "inverted nativists." The label "fascist" that was applied to them by many in the 1930s was not entirely appropriate, he says, for at least two reasons. First, the liberal and radical press used the term "fascist" so loosely and freely that it became more epithetical than descriptive. Second, the anti-Semitism preached by all three extremists—certainly, at least, by Coughlin and Pelley—had its roots in agrarian nativism and not Nazi Germany. The author believes it wrong to dismiss the Silver Shirts as a bunch of mental cases and seems to accept Pelley as a serious social philosopher whose nativism was "inverted." That is, he used alien symbols (the Nazis) to profess something American. If Father Coughlin and William Pelley led nativist movements under alien symbols, perhaps it can be said that Fritz Kuhn led an alien movement under nativist symbols.

Although Professor Smith's work is offered as an account of nondiplomatic factors that influence diplomatic history, there is little evidence that the three extremists he discusses had any influence on diplomatic history at all. He himself says that they were on the margins of events after Roosevelt's quarantine speech in 1937, although they did help to give isolationism a bad name. In the opinion of the author all three became victims of liberal intolerance that exaggerated the menace to American institutions against which they railed. This intolerance was strengthened by their own excesses and by events abroad. Before 1937 they were looked upon, in the main, as home-grown demagogues, after 1937 as traitors. In 1939 it was certainly difficult for them to maintain the thesis they held in common, namely, that an international Jewish-Communist conspiracy was dragging America into war. Smith says that by 1942 "the Bund had become a bad dream disturbing the sleep of particularly fitful New York Jewish-Americans" (p. 181). Since the "final solution to the Jewish question" was well under way in 1942, perhaps the bad dreams of the "particularly fitful" are understandable.

Smith speaks of liberal intolerance of the extreme Right. Cedric Belfrage and Stefan Kanfer (both journalists) expatiate on conservative intolerance of the extreme Left in the 1940s and 1950s. The congressional investigations of communism in American life that took place between 1945 and 1960 are summed up

by Belfrage in one big historical metaphor—the investigators were the Holy Office, cooperative witnesses and informers were “familiaris,” and those investigated were “heretics,” any difference between the Roman Catholic Holy Office and the democratic committees being “more apparent than real” (p. 29). The account of the investigations proceeds year by year with an occasional news miscellany (called a “Fever Chart”) in the manner of Dos Passos. It reads like a well-written summary of fifteen years of news as reported in the public press, and it is more heuristic than analytical. The master metaphor of Belfrage has been honored in such conservative writing as that of the late Willmoore Kendall, who said that procommunists and anticommunists were “quite right in venting upon the other the fury reserved for heretics, because each was, in the eyes of the other, *heretical*.” Belfrage and Kendall thus tend to validate the dictum of Clinton Rossiter that the Right and Left are closer to each other than either is to the center.

Kanfer concentrates upon the blacklisting in the 1950s of people in show business or, as he calls it, the “entertainment profession,” especially movies and broadcasting, and his treatment of events is also chronological. He writes in a fast-talking epigrammatic style, but the longitudinal sectioning of the material works against the best development of any particular story, which does not get told in one place. His book is knowledgeable, compassionate, and wry; critical of the booberies of blind political romantics and the hypocritical inhumanities of their persecutors; full of an insider's sense and sensibility about the entertainment world where commerce compromised both art and morality. It begins with Hollywood in the 1930s and ends with the successful suit against the blacklist by John Henry Faulk to whom a jury awarded damages of \$3,500,000, later reduced by the courts.

Although America in the Roosevelt, Truman, and Eisenhower years may have taken both Right and Left extremists more seriously than their numbers or influence warranted, still—it was then thought—it could ignore them only at the risk of losing its own sense of historic identity. With both Earl Browder and Fritz Kuhn claiming to speak for George Washington, the ideological controversies of three decades were an agonistic test of basic values. The sorrow is that so many innocents should have suffered.

EARL LATHAM  
Amherst College

CLARK A. MURDOCK. *Defense Policy Formation: A Comparative Analysis of the McNamara Era*. Albany: State University of New York Press. 1974. Pp. viii, 209. \$8.95.

Offhand, the title of this book would seem to indicate that the author intended to unearth the roots of policy making at the Pentagon, focusing on the role of Robert S. McNamara during his tenure as secretary of defense. In substance, however, it is far from a comprehensive inquiry into the underlying forces and considerations shaping McNamara's policies (including his own private interests and connections). Instead, the book is largely devoted to an examination of budget procedures and conflicts, systems for evaluating weapons, organizational structures and operations, and some theoretical problems of war gamesmanship.

The author bypasses the real and pulsating world of blood and flesh as he escorts the reader through a labyrinth of clusters of experts on war planning ensconced within the precincts of the Pentagon. Virtually insulated from the pressing life needs of the national community, these experts construct, at whatever the cost, the games that generals love to play.

Essentially the book tells us that McNamara was a top-flight efficiency expert determined to adopt a rational approach to the problems of war planning and weapons development. In contrast, Pentagon operations under President Eisenhower and to some extent under former Secretary of Defense Melvin Laird eschewed systems analyses, allowing the separate war services a larger measure of autonomy, which, in turn, bred bitter rivalries and the penchant for empire building.

The book steers away from probing the power exercised by private vested interests on policy making at the Pentagon. Eisenhower himself, nevertheless, was compelled to warn the public against growing ties between the military and influential corporate enterprises. Although the military budget soared under the rational operations of McNamara, the former secretary of defense appears to have experienced a “change of heart” after assuming the post of president of the World Bank. As reported in the *Christian Science Monitor* (September 22, 1970), McNamara deplored the fact that most countries were spending too much on the military. “For the so-called security of an ever-spiraling arms race,” he said, “the world is spending \$180 billion annually and the figure goes steadily up.” Noting that the richer nations hesitate to maintain adequate foreign

aid for the less affluent countries, McNamara then asserted: "That 20 times more should be spent on military power than on constructive progress appears to me to be the mark of an ultimate, and I sometimes fear, incurable folly."

It would be most enlightening to explore McNamara's apparent conversion. Here is a challenging assignment for Murdock.

HERBERT KRIEDMAN

*Nassau Community College*

JOHN HALL FISH. *Black Power/White Control: The Struggle of the Woodlawn Organization in Chicago*. (Studies in Religion and Society, Center for the Scientific Study of Religion.) Princeton: Princeton University Press. 1973. Pp. xii, 356. \$12.50.

A major concern of social and political scientists and urban specialists in the decade of the 1960s and into the 1970s has been the definition of community and the nature of community power. A recent overview of the field of community studies critically notes that much of this literature is "both normative prescription and empirical description" (Colin Bell and Howard Newby, *Community Studies* [1972], 218). This label aptly fits John Hall Fish's study of The Woodlawn Organization (TWO), which is based upon his six years of "intense involvement with TWO both as a scholar-observer and as an active participant." Fish maintains that TWO's approach to inner-city problems offers a "middle way" between increased dependence on technocratic urban bureaucracies and "chaotic and frustrating rebellion." His study is not only a description and a defense of TWO's history, but also a criticism of "the simplicities or impossibilities of the scholars" who write about urban problems. Fish argues that "the future of our deteriorating cities may depend less upon the brainwork of 'experts' than upon the spirit of the citizenry, less upon the grand designers than upon incalculable contingencies."

TWO arose in 1961 as a means to deal with the problems of social and physical deterioration facing the Woodlawn area located just south of the University of Chicago campus. The organizing spirit behind TWO was Saul Alinsky and his Industrial Areas Foundation, invited into the community by a group of local clergy. Alinsky's interpretation of the situation was that the plight of Woodlawn was largely the result of control by outside forces. To remedy this condition, Alinsky's professional organizers, using conflict tactics, developed a strong citizen's

organization to challenge the outside forces of control. Over the course of the 1960s, TWO took on, among others, the University of Chicago, the Chicago Board of Education, the Chicago Police Department, and Mayor Daley's political organization. The target of TWO action, according to Fish, was usually an agency or institution that was unresponsive to the interests and desires of the Woodlawn residents. Among TWO's achievements were the defeat of the university's plans for a South Campus, the closing down of 63rd Street's "Baby Skid Row," and the development of a successful job-training program. The issues TWO lost, such as a highly controversial youth-training program using local gangs (the Blackstone Rangers and the Disciples), the Woodlawn Experimental Schools Project, and control of a model cities program, were those that involved a direct challenge to established power groups like the city agencies. In order to continue to exist, TWO was forced to work within the constraints of the existing political process even though the solution to Woodlawn's worst social problems demanded significant alterations in this process. By 1971 TWO had become much more of a community development corporation than a militant citizen's group. Given Fish's interpretation that "continued existence is TWO's vision," this was a move of "pragmatic realism."

It is difficult to evaluate Fish's arguments given the tremendous controversy generated by the community action programs of the 1960s. I tend to find Fish's conclusions and those of other advocates of community control, such as Alan A. Altshuler, persuasive given the existing alternatives. From a larger perspective, this work highlights some of the continuing dilemmas of reform in American cities. What kind of balance can be reached between the needs of local self-determination and centralized direction? How can the voices of outside "experts" be harmonized with those from the neighborhoods? And, perhaps most basic given the white, middle-class orientation of American institutions and society, can the process of reform, even if based on local self-determination, bring about substantial changes in the positions of the poor and the minorities?

JOEL A. TARR

*Carnegie-Mellon University*

STEPHEN STEINBERG. *The Academic Melting Pot: Catholics and Jews in American Higher Education*. A report prepared for the Carnegie Commission on Higher Education. New York: Mc-

Graw-Hill Book Company. 1974. Pp. xx, 183. \$8.95.

This is a highly useful and perceptive study of a question much mooted in faculty clubs and scholarly journals in the past two decades. Commissioned by the Carnegie Commission, it has very considerable virtues that outweigh its three major weaknesses. Why obfuscate what is a careful statistical and logical analysis with a title about the academic melting pot? Nathan Glazer and Daniel P. Moynihan have, I am afraid, started a fad by entitling a book *Beyond the Melting Pot* (1963), which they then show never existed. Second, Steinberg makes only the most tenuous of connections between the historical-philosophical analysis of Jewish and Catholic traditions—for those are what he is talking about—and the careful statistics of present conditions in America. Third, he gives us nowhere, in this much-tabled book, a list of the “ranking” universities upon which many of his conclusions depend.

These caveats aside, it is a most useful book, though not to someone who does not like figures, or who prefers single-correlation analysis, or who does not like to consider alternative hypotheses for explaining complex data. It is with considerable trepidation that I underline some of his findings. First, the statistical overrepresentation of Jews in higher education applies only to some fields if inquiry. Second, this is the result more of the fact that when the explosion of higher education took place in this country just after World War II, there were more Jews, proportionately, in middle- and upper-class positions than either Catholics or Protestants (especially if one recollects, as Steinberg carefully does, that not all Protestants come from Scarsdale and go to Exeter and Swarthmore; quite a few live in Appalachia, or in areas where literacy is likely to be thought a bit uppity). Third—and here I perhaps go beyond Steinberg—deep religiousness provides a kind of alternative expenditure of energy to the pursuit of higher education and scholarship, measured by the number of Ph.D.'s and articles in *Chemical Abstracts*, or for that matter the *American Historical Review*.

I think I read Steinberg correctly when I say he suggests that the debate about Jewish intellectualism and Catholic anti-intellectualism may now be laid to rest, in the prospect that, within the range of ethnic difference, in another generation Catholics, Jews, and most Protestants will be about proportionately represented in graduate school, in faculties—even in Steinberg's somewhat mysterious “ranking” universi-

ties. That there will be more of one group than another in biology, say, than in music history does not seem at all depressing; the melting pot, even as advocated by its most enthusiastic supporters, never assumed that one-third of all bassoon players ought to be Baptists.

ROBERT D. CROSS

*University of Virginia*

## LATIN AMERICA

H. S. FERNS. *The Argentine Republic, 1516-1971*. (National Economic Histories.) New York: Barnes and Noble. 1973. Pp. 212. \$10.00.

Professor Ferns, a British scholar specializing in Argentine economic history, presents here a useful if sometimes analytically uneven monograph.

In a mere 180 pages of text the author seeks to trace Argentina's economic development from 1516 to 1971. The suspicion that to attempt such a large task in such a small space may result in commonplace if not superficial and uneven treatment of portions of the subject is borne out. The entire colonial economy is treated in ten pages. The proportions improve relatively thereafter, but a serious question may be raised about analysis. Is a reviewer overly suspicious (again) in wondering if the absence of a critical treatment of Argentina's economic dependency on Great Britain down to the 1930s is linked to the author's appreciation of the long and once warm relations between the author's native land and the commercial and landed elites of Argentina? (This is not to say that Professor Ferns does not refer to some of the social and political cost of dependency: he does so on page 70, for example.)

The latter chapters, which deal with Argentina on its own, so to speak, in the period of forced industrialization under Perón and later governments, are much more critical, not only of the process of industrialization in a land so clearly fitted for export agriculture, but critical particularly of the failures of Argentina's political and economic leaders to resolve fundamental internal contradictions. Here Professor Ferns writes with biting cynicism in phrases such as “the administrative sadism of [Argentine] state agencies” and in such conclusions as the following: “The observable contradictions of the Argentine economy—spare capacity and under-used industrial plants, over-used and worn-out equipment, large wage increases and unemployment, exceedingly low prices in some industries such as electricity generation and very high

prices in others such as motor vehicles, negative rates of interest and high profits—are thus explicable but indefensible” (pp. 184–85).

Another substantive problem of the book is the difficulty of determining the audience at which it is aimed. The analysis is generally too much derived from published works (and clogged with statistics that should have been placed in appendixes) to be of prime value to scholars familiar with Argentina. Yet it assumes more knowledge about Argentina and is sometimes too technical in its treatment of economic matters to be easily assigned to undergraduate Latin American courses—at least in history—or valued by the general reader.

While one may wish that Professor Ferns had dedicated his talent to a more original study of Argentina's twentieth-century economy, parallel to his highly successful book on Great Britain and Argentine economic relations in the nineteenth century, his critical treatment of Argentina's economic muddles of the past forty years is worth having as the only presently available synthesis that attempts to explain the strange economic life-style of that rich but poor country.

THOMAS F. MCGANN  
University of Texas,  
Austin

KENNETH R. MAXWELL, *Conflicts and Conspiracies: Brazil and Portugal, 1750–1808*. (Cambridge Latin American Studies, 16.) New York: Cambridge University Press, 1973. Pp. xix, 289. \$14.95.

A celebrated event in Brazilian history is the abortive conspiracy of Minas Gerais (*Inconfidência Mineira*) in 1788–89. For textbook or civic purposes it is depicted as a prophetic insurrection of patriotic literati inspired in part by the ideals of the American Revolution. The father of modern Brazilian historiography, Capistrano de Abreu, dismissed the episode as unworthy of serious notice. Contemporary historians have called it the work of “romantic dreamers” (E. Bradford Burns, 1970) and “a bizarre plot that should have been put down quickly and forgotten” (Donald E. Worcester, 1973).

After a painstaking reconstruction of its origins and denouement, Kenneth Maxwell concludes that the *Inconfidência*, far from being a quixotic gesture, was of critical importance for the colonial oligarchy and for Portuguese

imperial policies. He identifies three levels of support for the movement: the “activists,” including the *alferes* (lieutenant) Silva Xavier, or *Tiradentes* (Tooth-puller), hanged as the scapegoat, and the poet-magistrate-landowner Alvarenga Peixoto; the “ideologues,” including poet-magistrate-political theorist Tomás Antônio Gonzaga and poet-lawyer-businessman Cláudio Manuel da Costa, presumed translator of Adam Smith; and a third, “shadowy” group of contractors and merchant-capitalists, designated as the “financial interests.” The last group, essential to the analysis, linked the conspiracy to the “regional power structure.” Maxwell finds that the conspirators’ goal of independence from Portugal subsumed the generally conservative social and economic agenda of a “coalition of magnates.”

In colonial Latin America bureaucratic and commercial urban interests were often protected by Spain and Portugal. This partly accounts for the “ruralization” of power structures when independence cut the ties with the metropolis, withdrawing overseas support for urban groups. Minas Gerais, however, had produced an internally knit, relatively urbanized regional economy whose elites resented Portuguese neomercantilism. Privileged Brazilians and local peninsulars were willing to challenge the metropolis head on, a situation rare in the Iberian colonies.

Although Maxwell's story hinges on the *Inconfidência*, its scope is much broader, both temporally and spatially. The early chapters trace growing tensions in the Luso-Brazilian imperial system in the 1770s and the complex involvement of British commerce in the process. His conclusion contrasts the *Inconfidência* with the 1798 conspiracy of the mulatto artisans of Bahia, showing that the frustrated republicanism of one and the frustrated populism of the other elicited compromise attitudes in Portugal and its colony that later eased the transfer of the Portuguese court to Rio de Janeiro in 1807–08.

With its solid research core and broad vistas, Maxwell's is in many ways a model study of a society for which reliable monographs are sparse. One yearns, however, for occasional relief from the implacable quest to pinpoint economic interests and coalitions. The book pays no tribute to Antônio Cândido's sensitive literary studies of the Minas ideologues or to the exegesis of Gonzaga's treatise on natural law and the explorations of Minas baroque art as a political statement by Lourival Gomes Machado. Today's insistence that colonies are mere periphery



obscures the fact that even a colony may, for those who live there, become a center.

RICHARD M. MORSE  
Yale University

LUIS GONZÁLEZ. *San José de Gracia: Mexican Village in Transition*. Translated by JOHN UPTON. (The Texas Pan American Series.) Austin: University of Texas Press. 1974. Pp. xxviii, 362. \$12.50.

A welcome trend in recent Mexican historiography is the appearance of solid regional and local studies that serve to balance off the time-honored tradition of sweeping overviews and institutional monographs. *San José de Gracia*, winner of the AHA's first C. H. Haring Prize for the outstanding historical work written by a Latin American, heads the short list of penetrating, humanizing local histories. First published in Spanish in 1967, this English translation of the revised second edition (1972) retains the crisp style and clarity of the original.

Luis González, historian, and the Michoacán town of San José de Gracia are natural partners. González is both a highly regarded professional historian based in Mexico City's prestigious Colegio de México and a native son of San José. This happy combination of professional training, internalized knowledge of the community, and access to the intimate thoughts of common people help the author weave much revealing folk history—the people's unwritten perceptions of their past—and official documentation into a personalized story of the community since its founding in the late nineteenth century. González is never in danger of succumbing to the antiquarian's bent for elaborate, unconnected details. His skillful handling of local, regional, and national contexts allows the outsider as well as the local citizen to view the people of San José on several levels simultaneously.

Since completing this study, González has described his approach more explicitly in *Invitación a la microhistoria* (1973). He proposes the term "microhistory" in place of "local history" because "the important thing is not the size of the place but the smallness and cohesiveness of the group under study, the minuteness of the things that can be said about the group, and the myopia that helps bring them into focus" (*Invitación*, p. 11). That *San José de Gracia* has a "feel" similar to the novels and stories of Agustín Yáñez should not be surprising. For González, microhistory and litera-

ture are twin sisters. Microhistory "springs from the heart not from the head. . . . [It] is the individualized story of humble people, the local citizens who rarely appear in the society page of the newspapers and almost never in the political or economic sections. . . . Local examples are indigestible unless artistic emotion can bring them to life; unless they are given the taste, smell, and music of living things" (*Invitación*, pp. 54, 57, 58).

An especially striking insight revealed by González's organic approach is that, in spite of obvious continuities of environment and population, the history of San José seems to divide quite emphatically into a series of very different generations: the "angry" generation of citizens born between 1879 and 1890 who grew up in adversity and were prepared to take their chances in the Cristero rebellion of the late 1920s; the "enterprising" generation (born 1905–17) who have actively sought acculturation; the "uprooted, fault-finding" generation (born 1920–34), a weaker and more ambitious lot; and most recently, the "individualists" with "no inclination toward loyalty, gratitude, or affection" and an adolescent group that seems "happy, robust, submissive." González relates the character of the earlier generations to natural conditions, especially calamities ("Microhistory is almost always geohistory," he says in *Invitación*, p. 56), while the newest groups seem to be increasingly shaped by the political and social worlds outside the district of Jiquilpan. He leaves us with a community that in 1972 hangs in the air, "suspended, but not necessarily insecure" (p. 339).

WILLIAM B. TAYLOR  
University of Colorado,  
Boulder

DONALD EDMUND RADY. *Volta Redonda: A Steel Mill Comes to a Brazilian Coffee Plantation. Industrial Entrepreneurship in a Developing Economy*. [Albuquerque: Rio Grande Publishing Company.] 1973. Pp. xii, 380.

To give credence to the fact that Brazil may become a leading industrial nation of the world, Dr. Rady has encapsulated a history of Brazilian industrial growth by way of this monograph on the "Steel City," Volta Redonda. Volta Redonda, according to the author, seems to herald a final awakening of a so-called sleeping giant. Chapter by chapter the author documents well this growth and development of Brazil's National Steel Company. It is a story

from the ground up of twentieth-century industrial growth of a steel city where only a fazenda existed. The headline reads in chapter 8: "Volta Redonda: Farm Village to Factory Town."

The text of the first three chapters contains imposing statistical reports and surveys of the geographical and geological setting sufficient for a government assay office. In dissertation form the text lists and graphs the past, present, and future possibilities of the area for industrial growth. The author then discusses the role of former President Getulio Vargas as a catalytic agent in literally "grabbing at straws" for investment and expertise until his steel mill dream became reality.

After accounting for most of the criteria for developing and making operational this National Steel Company, Dr. Rady describes in a more personal vein "The Company as an Industrial Institution" (ch. 7) and its influence on the social institutions of the new city. He discusses the migration of "strangers" to the area and the problems of assimilation.

Finally, chapter 11 indicates the problems of impact on the Brazilian economy: the investments, capital, labor, machinery, raw materials, and social services. Especially noted is the data and discussion on Brazilian auto production.

Of special importance is the bibliographical essay, endnotes to bibliography, and selected bibliography for further research. This seems to be a research coup for Rady, for he states that there has always been difficulty in obtaining reliable statistics and studies. Both the government and the company cooperated in aiding him in his research. In this instance, it is interesting that Rady notes a special kind of government-company paternalism in policy making. He also notes the preponderance of military executive personnel, from the company president to labor supervisors.

My critical note concerns the use of footnotes. Many of the footnote discussions should have been an integral part of the text. A footnote here, however, should state that Rady has written a clear, concise account of a "new era" in Brazilian economic history complete with illustrations.

ORVA LEE ICE

*Macomb County Community College*

PATRICIA W. FAGEN, *Exiles and Citizens: Spanish Republicans in Mexico*. (Latin American Monographs, number 29. Institute of Latin American Studies, University of Texas at Austin.)

Austin: University of Texas Press, for the Institute. 1973. Pp. x, 250. \$8.00.

This is a very sound treatment of the Spanish Republicans living in Mexico in their dual capacity as exiles from Spain and as citizens of the country that gave them refuge. An able researcher with a deep interest in the people she studies, Dr. Patricia Fagen has combined thorough use of documentary sources with extensive interviewing in a successful effort to reconstruct the experience of those supporters of the Spanish Republic who migrated to Mexico following their political-military defeat at the hands of Franco's forces. Dealing equally well with the historical aspects of the Spanish Republic and the process by which many intellectuals and professional men were rescued from its collapse on the one hand and the mutual adjustment of Spaniard to Mexican on the other, she also handles contemporary social and political facets of the problem with sensitivity and a reasonable degree of sophistication. In brief, her thesis is that the Spanish exiles both remained part of the liberal tradition of their homeland and became a significant factor in Mexico's intellectual and social life. Yet they could not participate actively in the creation of the new political order emerging from the Mexican Revolution. Thus their contribution was much more to the economic development of their new home than to its political progress. As a result, "Over the years the exiles, especially in the older groups, have continued to be psychologically torn by the intensity of their loyalty to Spain, both as their homeland and as the political ideal for which they went into exile, and their loyalty to Mexico, which for so long has been their adopted country."

The author is perhaps at her best in the insightful analysis of exile politics (pp. 105-44), where she points out the sharply declining importance, or even relevance, of such activity. Not only have the children of the exiles shied away from their parties and movements, but some of the older leaders have accommodated to representatives of the Franco regime for a variety of motives Professor Fagen sensitively explores. Regarding prospects for returning to Spain, she perceptively outlines the exiles' dilemma: "The exile, once political, is now as much, or more, an economic exile. The prosperity which the exiles have achieved in Mexico, coupled with the considerably more limited opportunities for work in most fields in Spain, proves a more effective deterrent to the Republicans' return than does the Spanish Government."

Through her skillful use of interviews, the author is able to probe the attitudes involved in the exiles' ambivalent feelings concerning their second fatherland. Here she also utilizes literary works as a supplementary source on feelings and perceptions.

A final positive aspect of this study is the attempt embodied in it to put this phenomenon into a comparative perspective, underscoring

both similarities and differences with other exile groups. Her conclusions deserve to be read and pondered, not summarized in a review, which in honesty can be said of far too few books these days.

RONALD SCHNEIDER  
*Queens College,*  
*City University of New York*

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## Communications

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*A communication will be considered only if it relates to an article or review published in this journal; publication of such a communication or of any part of it is solely at the editors' discretion. Limitations of space require that a communication, whether concerning articles, review articles, or reviews, be no longer than 300 words. The schedule of publication and the time needed to send a communication to the author of the article or review in question for such reply as he may care to make virtually preclude the possibility of publication in the issue following that in which the original article or review appeared. Unless, in the editors' judgment, some major scholarly purpose is served, rejoinders will not be published.*

### TO THE EDITOR:

In his review of the three volumes in the Borzoi Series in United States Constitutional History (*AHR*, 79 [1974]: 1257-58), Jon Teaford criticizes the authors for their narrow preoccupation with individual case histories, and suggests that broader studies of such topics as judicial review, the impact of changes in transportation and economic structures on judicial development of the commerce power, or the constitutional development of the 1930s as a unit, would have been more desirable. While this represents a legitimate value judgment, it misses the point of the purpose of the series. As its editor, I was persuaded that what students of constitutional history did not need was further broad amorphous studies of judicial review or the commerce clause; but that through the vehicle of selected key cases, constitutional history could be retrieved from those who view it only as explorations of the rendering of cases, statutes, and principles, and it could be better related to those societal factors that shaped the law.

Thus Professors Dewey, Baxter, and Cortner were asked to prepare volumes on those terms with that latter purpose in mind. In the conceptualization of the series it was felt that with enough such careful studies, new insights could be gained into the general history of law and society in the United States. Conversely, it was felt that an orientation that called for simply exploring the broader factors influencing and determining the course of American constitutional history would merely add further imprecise and speculative volumes to the many of that genre which already exist.

PAUL L. MURPHY  
University of Minnesota,  
Twin Cities

### MR. TEAFORD REPLIES:

I understand Professor Murphy's comments concerning the Borzoi Series in United States Constitutional History and recognize that the case-study approach may be one possible means of examining American constitutional development. Yet I still believe that the series might have proved more useful had it focused on the evolution of constitutional doctrine rather than on the particulars of landmark decisions. Constitutional historians have too often presented a simplistic image of the United States Supreme Court handing down momentous decisions and thus "making" American history. Students should recognize the social, economic, and intellectual forces that mold constitutional development and not simply the dramatic outlines of single cases. They should realize, for example, that the doctrine of judicial review is not merely the product of a feud between Jefferson and Marshall but an outgrowth of late eighteenth-century political philosophy. The authors of the Borzoi series have provided some insight into the broad forces transform-

ing American law, but I believe that they have concentrated too heavily on the evolution of specific litigation and too often ignored the general course of constitutional development.

Moreover, I do not believe that volumes which reject the case-study approach need be amorphous or hopelessly speculative. Intelligent speculation on the relation of law and society might be a great deal more useful and stimulating to a student than excessive detail. I have never viewed history as a precise science, and both teachers and students should welcome the enlightened speculation of such accomplished scholars as Professors Dewey, Baxter, and Cortner.

JON C. TEAFORD  
Iowa State University

#### TO THE EDITOR:

Although Professor Polk had some kind words to say about my book *The Question of Palestine, 1914-1918* (*AHR*, 79 [1974]: 1226-27), on the whole his evaluation seems to me gratuitous. Contrary to his observations, my book was written against the background of British war aims in the East (see chs. 2, 10), and the circumstances in which decisions were made by the British statesmen were described in great detail. The *ad hoc* nature of the decisions taken (though not all!) is clearly evident from the relevant chapters, and even a cursory reading will suffice to show that complaint on this count is unjustified.

I had no preconceived "objective"; the documents alone led me to the conclusion that there was no inconsistency between the McMahon-Husain understanding and the Sykes-Picot Agreement and that there was no incompatibility between the former and the Balfour Declaration. Nor was I oblivious, as Polk says, of the "problems" that resulted from British commitments. I specifically alluded to their origin (see pp. 72, 95-96, 116-18, 209-10, 222-25, 226, 303-04, 330-32 of my book). These problems,

however, did not surface until the Peace Conference in Paris in 1919, a period that lay outside the scope of my book, though I hope to deal with it elsewhere.

Lord Curzon's statement on December 5, 1918, does not contradict my thesis at all. Misled by Toynbee, he soon afterward corrected his mistake (see pp. 89-90, 94-95). By quoting a statement from Balfour's memorandum of August 11, 1919, Polk fell into the same trap as some other historians did, for Balfour, in this case, was under the misapprehension that the Anglo-French declaration of November 1918 and the 1919 Covenant related to Palestine, whereas all the documentary evidence points to the contrary. Balfour himself resolved the "contradiction" in his speech in the House of Lords three years later. His query with regard to the King-Crane Commission had greater validity, for its purpose was ill conceived and President Wilson wisely suppressed its conclusions.

It is true that in 1923 Grey had some "guarded thoughts," but in March 1916, as foreign minister, when confronted with an identical problem, he gave his unqualified approval to an even stronger formula than the Balfour Declaration (pp. 55-58) and in December 1917 warmly welcomed it (Leonard Stein, *The Balfour Declaration* [1961], 41 ff., 114). At any rate, the original sponsors of this policy had no regrets (pp. 331-32 of my book). This evidence was completely ignored by the reviewer. Another example of distortion: on page 65 I wrote: "Today it has no more than an academic value . . ."; "it" clearly referred to the controversy over the McMahon-Husain correspondence and whether or not Palestine was covered by the British promise. But Polk inserted after "it" the words [the Palestine question] thus giving the impression that my statement related to a broader issue, which was completely untrue.

ISAIAH FRIEDMAN  
The Dropsie University



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## Recent Deaths

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To write of JACOB LARSEN, who died in Columbia, Missouri, on September 2 at the age of eighty-six, is to recapture a distinguished scholar and gentleman. Happily his friends will immediately clothe the bare skelton of his career with the flesh and blood of his character and personality. He took his A.B. at Luther College in 1908 and his A.M. at the University of Iowa in 1910. He attended Yale University, 1910–11, whence he proceeded as Rhodes scholar to Oxford where he received his B.A. in 1914 and his M.A. in 1920. In those intervening years he served the United States in Denmark. Returning to the United States he took his Ph.D. at Harvard in 1928. Meanwhile he had taught at the University of Washington from 1920 to 1926 and had moved to Ohio State University where he taught from 1926 to 1930. From there he went to the University of Chicago where he remained from 1930 until his retirement in 1953. In 1953–54 he was Sather Professor of Classics at California. He also taught for brief periods at Columbia University, Northwestern, the University of Michigan, the University of Texas, Rutgers, and finally at the University of Missouri from 1960 until his second retirement in 1971. "Retirement," however, is no word to apply to Jacob Larsen: he never retired. To the very end he was learning, he died learning, adhering to the same standards that had always characterized his work. Those standards, however rigorous, were modestly stated. In the introduction to his *Greek Federal States: Their Institutions and History* (1968), the culmination of nearly half a century of inquiry, he declared his hope, which was also his creed, that the work, though far from perfect, would "prove helpful to future students."

He wore his many honors lightly, whether his Sather Professorship from which came his *Representative Government in Greek and Roman History* (1955), his managing editorship of *Classical Philology* (1939–51), his presidency of the American Philological Association (1952),

his honorary degrees from the University of Vermont (1953) and Luther College (1961), or his election as corresponding fellow of the British Academy (1967). Alongside these testimonies to his quality were others less tangible but equally, perhaps more, meaningful: the graceful homage of two great scholars, also Sather Professors, Arnaldo Momigliano, whose learning knows no bounds, and Frank Walbank, more the specialist but no less the humanist, who went out of their way to pay visits to the man they acknowledged a master. To specify these two men—and many others might be cited—is also to accent Larsen's ability to detect merit on the one side, its absence on the other. Kind, generous, but never mealy, he would occasionally inquire if I had read the work of certain individuals; then, after a moment, scarcely longer than that necessary for a breath, might come the crusher, "Now, what in the hell do you suppose he's wasting his time on that stuff for?" Never spiteful, he simply had no patience with what he deemed unworthy. His teaching at every level reflected the man and the scholar. No one sat in his classes without appreciating that he had been in the presence, not of pomp and circumstance, but of distinction, and many there are to testify to that quality. They knew what one colleague put into words: "He always seemed to me the model of what a university professor *should* be." What more need be said?

CHARLES F. MULLETT  
*University of Missouri*

BERT J. LOEWENBERG, professor of history emeritus at Sarah Lawrence College, died on August 13, 1974, at his home in Orange, Connecticut. Born in Boston, Massachusetts, on December 24, 1905, Loewenberg received his undergraduate training at Clark University, where he also earned an M.A. He was awarded a second M.A. and a Ph.D. at Harvard University and there wrote his seminal dissertation on

the influence of Darwinism in American thought, subsequently published in a series of now classic scholarly articles. For most of his scholarly career he remained engaged with the history of ideas, especially in the nineteenth or, as he called it, Darwin's century.

During the Great Depression, Loewenberg directed the Massachusetts Federal Writers' Project; in the latter part of the decade he taught American history as an assistant professor at the University of South Dakota. In 1942 he was appointed professor of history at Sarah Lawrence and remained there for the next thirty years, exercising a central influence on the institutional development of the college. A profound, pragmatic concern with methods of education led him to elaborate some of the distinctive aspects of the college's curriculum, including its emphasis on independent studies and tutorials. Though he liked best such individual sessions with students, he was often required to teach huge lecture courses; most undergraduates regarded his always overcrowded survey course, "American Life and Thought," as one of the indispensable parts of a Sarah Lawrence education. In his advanced seminar, "Ideas in America," Loewenberg taught several generations of students the charms and intricacies of intellectual history, and in this setting evolved many of the themes of his last published work, *American History in America Thought* (1972).

Loewenberg's scholarship ranged over vast areas of history and philosophy. His prose often revealed a man of strong public passions and cosmopolitan tastes. But at the heart of his interest in history, science, and education lay a deep and simple Jeffersonian commitment to the idea that an informed citizenry is the only safeguard of democracy.

Among his numerous honors and awards, Loewenberg prized his fellowship in the Royal Historical Society, his honorary LL.D. from Clark University (1970), and his chairmanship in 1957-59 of the American Darwin Anniversary Committee. Loewenberg held visiting chairs at a dozen universities including Cornell University (1947); Ruskin College, Oxford (1952); Hebrew University (1953); Leeds and Cambridge (1960-61).

CARL RESEK

*State University of New York,  
College at Purchase*

S. EVERETT GLEASON, who died in Washington on November 20, 1974, was an experienced government official as well as a fully qualified historian. Born in Brooklyn on March 14,

1905, he took his academic degrees (A.B. [1927], A.M. [1928], Ph.D. [1934]) at Harvard, where he concentrated in European medieval history, studying under Professor Charles H. Taylor and under the late Charles H. McIlwain and George La Piana. In 1932 he held a Sheldon fellowship that enabled him to devote a year of postgraduate study at the Ecole des Chartes. His dissertation, *An Ecclesiastical Barony of the Middle Ages*, was published as a Harvard Historical Monograph in 1937.

Dr. Gleason married Mary Eleanor, the daughter of the late Wilbur Cortez Abbott, in 1937 when he was serving as an instructor in history at Harvard (1930-38). Thereafter he held the positions of assistant and associate professor of history at Amherst College (1938-46), though most of that period was spent on leave in Washington as a member of the staff of the Research and Analysis Branch of the Office of Strategic Services (1943-45). Commissioned as a major and later promoted to lieutenant colonel on the staff of the Joint Chiefs of Staff, Dr. Gleason was awarded the Legion of Merit at the termination of his service.

Dr. Gleason's major historical work was done in collaboration with the present writer. The two friends, sponsored by the Council on Foreign Relations, undertook, on the basis of a four-year grant, to produce a scholarly account of the development of United States foreign policy from about 1937 to the end of the Second World War. Since they were personally well known in Washington, they were accorded official support to an unusual degree. They were given full access to the State Department archives, the reports of the OSS so far as relevant to their project, the Morgenthau and many other private papers, and interviews. Despite Herculean efforts, however, the mass of source materials, both American and foreign, archival and published, proved too great to permit adequate coverage of more than the period preceding the Pearl Harbor attack and the entrance of the United States into the war. These five years were, however, treated on a global scale, setting American problems and policies into the world frame. The result was two volumes, *The Challenge To Isolation* (1950) and *The Undeclared War* (1953), of which the latter was awarded the Bancroft Prize.

Dr. Gleason continued to work on this project after the original grant had expired and he had taken a position as deputy executive secretary of the National Security Council (1950-59). He was then appointed cultural attaché to the United States Embassy in London and, on his return to Washington after two years, joined

the Department of State, where he served in the Historical Office until his retirement in 1972. As deputy chief of the Foreign Relations Division he had chief responsibility for the successive volumes of *Foreign Relations of the United States*.

Dr. Gleason was an elected member of the Massachusetts Historical Society, the Colonial Society, and the Council on Foreign Relations. He belonged also to the Century Association (New York), the Cosmos Club (Washington), and the Athenaeum (London). He was a first-rate scholar, an expert in problems of foreign policy, and a gentleman of great personal charm and complete devotion to the service of his country.

WILLIAM L. LANGER  
*Harvard University*

ALLEGRA WOODWORTH, head of the history department at the Shipley School in Bryn Mawr from 1927 until her retirement in 1971, died suddenly of a heart attack at her home in Haverford, Pennsylvania, on December 27, 1974. She was seventy-two years of age.

Miss Woodworth graduated from Girls' High School in Philadelphia and received her A.B., M.A., and Ph.D. degrees from Bryn Mawr College. She was a scholar in Tudor history known in England and America for her authoritative book *Purveyance in the Royal Household of Queen Elizabeth (1558-1603)*, published in the Transactions of the American Philosophical Society in 1945. At the time of her death she was working on a study of William Fleetwood, recorder of London (1571-93).

## Other Books Received

Books listed were received by the *AHR* between December 1, 1974, and February 1, 1975. Books that will be reviewed are not listed, but listing does not preclude subsequent review.

### GENERAL

- ALLAIN, MATHÉ, and CONRAD, GLENN R. (eds.). *France and North America: The Revolutionary Experience*. Proceedings of the Second Symposium of French-American Studies, March 26-30, 1973. Lafayette: USL Press. 1974. Pp. xiv, 260. \$10.00.
- ANDERSON, PERRY. *Passages from Antiquity to Feudalism*. Atlantic Highlands, N.J.: Humanities Press. 1974. Pp. 304. \$15.00.
- BADURA, BERNHARD, and GLOY, KLAUS. *Soziologie der Kommunikation: Eine Textauswahl zur Einführung*. Problemata, no. 11. [Stuttgart:] Frommann-Holzboog. 1972. Pp. 352.
- BOWDEN, ELBERT V. *Economics Through the Looking Glass*. San Francisco: Canfield Press. 1974. Pp. viii, 163. \$2.95.
- BRAVERMAN, HARRY. *Labor and Monopoly Capital: The Degradation of Work in the Twentieth Century*. Foreword by PAUL M. SWEETZ. New York: Monthly Review Press. 1975. Pp. xiii, 465. \$12.50.
- CLAPP, PRISCILLA, and HALPERIN, MORTON H. (eds.). *United States-Japanese Relations: The 1970's*. Cambridge, Mass.: Harvard University Press. 1974. Pp. 234. \$7.95.
- COCKBURN, CLAUD. *The Devil's Decade: The Thirties*. New York: Mason & Lipscomb. 1973. Pp. 254. \$8.95.
- DUKER, ABRAHAM G., and BEN-HORIN, MEIR (eds.). *Emancipation and Counter-Emancipation: A Jewish Social Studies Reader*. Selected Essays from *Jewish Social Studies*. With an introd. by SALO W. BARON. New York: Ktav Publishing House. 1975. Pp. ix, 413. \$15.00.
- ENZENSBERGER, HANS MAGNUS. *Politics and Crime*. Selected by MICHAEL ROLOFF. Continuum Book. New York: Seabury Press. 1974. Pp. 215. \$8.95.
- FROMKIN, DAVID. *The Question of Government: An Inquiry into the Breakdown of Modern Political Systems*. New York: Charles Scribner's Sons. 1975. Pp. 228. \$8.95.
- HARTMAN, MARY S., and BANNER, LOIS (eds.). *Clio's Consciousness Raised: New Perspectives on the History of Women*. Harper Torchbooks. New York: Harper & Row. Pp. xii, 253. \$3.95.
- HAUPT, GEORGES, et al. *Les marxistes et la question nationale 1848-1914: Études et textes*. Bibliothèque Socialiste, 27. Paris: François Maspero. 1974. Pp. 391.
- HOOKE, SIDNEY. *Pragmatism and the Tragic Sense of Life*. New York: Basic Books. 1975. Pp. xvi, 224. \$12.50.
- KITSON, FRANK. *Low Intensity Operations: Subversion, Insurgency, Peace-keeping*. Hamden, Conn.: Archon Books. 1974. Pp. xi, 208. \$9.00.
- LANGER, WILLIAM L., et al. *Western Civilization*. Vol. 1, *Prehistory to the Peace of Utrecht*; vol. 2, *The Expansion of Empire to Europe in the Modern World*. 2d ed.; New York: Harper & Row. 1975. Pp. xiii, 526; xiii, 485. \$7.95 each.
- LUNDBERG, MARGARET J. *The Incomplete Adult: Social Class Constraints on Personality Development*. Contributions in Sociology, no. 15. Westport, Conn.: Greenwood Press. 1974. Pp. xv, 245.
- LYON, E. WILSON. *Louisiana in French Diplomacy 1759-1804*. Norman: University of Oklahoma Press. 1974. Pp. 268. Cloth \$8.95, paper \$3.95.
- MCNEILL, WILLIAM H. *The Contemporary World: 1914-Present*. Scott, Foresman World Civilization Ser. Rev. ed.; Glenview, Ill.: Scott, Foresman. 1975. Pp. 184. \$3.95.
- MEE, CHARLES L., JR. *Meeting at Potsdam*. New York: M. Evans & Co. 1975. Pp. xiv, 370. \$10.95.
- MOSSE, GEORGE L. *The Culture of Western Europe: The Nineteenth and Twentieth Centuries*. Rand McNally History Ser. 2d ed.; Chicago: Rand McNally College Publishing Co. 1975. Pp. 410. \$6.95.
- NAROLL, RAOUL, et al. *Military Deterrence in History: A Pilot Cross-Historical Survey*. [Albany:] State University of New York Press. 1974. Pp. lxii, 416. \$20.00.
- PACAUT, MARCEL, et al. *Le monde contemporain, 1945-1973*. Paris: Armand Colin. 1974. Pp. 391.
- PARKER, HAROLD T., and BROWN, MARVIN L., JR. *Major Themes in Modern European History: An Invitation to Inquiry and Reflection*. Vol. 1, *The Institution of the State*; vol. 2, *The Institution of Liberty*; vol. 3, *The Twentieth Century*. Durham, N.C.: Moore Publishing Co. 1974. Pp. 300; 301-641; 642-1122. \$3.75 each.
- POULANTZAS, NICOS. *Political Power and Social Classes*. Tr. and ed. by TIMOTHY O'HAGAN. Atlantic Highlands, N.J.: Humanities Press. 1975. Pp. 367. \$16.50.
- Prace Historyczne* [Historical Studies], 50. Zeszyty Naukowe Uniwersytetu Jagiellońskiego, 364. Cracow: Jagellonia University. 1974. Pp. 124. Zł. 18.
- REXROTH, KENNETH. *Communalism: From Its Origins to the Twentieth Century*. Continuum Book. New York: Seabury Press. 1975. Pp. xviii, 316. \$12.95.

- ROMEIN, J. M. *Apparaat voor de studie der geschiedenis* [Tools for the Study of History]. New ed. by J. HAAK. Rev. by J. G. F. HASEKAMP. Groningen: H. D. Tjeenk Willink. 1974. Pp. 221.
- RUDHART, ALEXANDER. *Twentieth Century Europe*. Philadelphia: J. B. Lippincott. 1975. Pp. xvi, 656. \$12.95.
- RUSH, MYRON. *How Communist States Change Their Rulers*. Ithaca: Cornell University Press. 1974. Pp. 346. \$15.00.
- SALOMON, ERICH. *Portrait of an Age*. New York: Collier Books. 1975. Pp. xiv, 221. \$7.50.
- SCHOLBERG, HENRY. *Bibliographie des français dans l'Inde*. Pondicherry, India: the Historical Society; distrib. by InterCulture Associates, Thompson, Conn. 1973. Pp. lxiv, 216. \$11.50.
- SEARLE, G. W. *The Counter Reformation*. Totowa, N.J.: Rowman and Littlefield. 1975. Pp. 189. Cloth \$7.50, paper \$3.75.
- STEINERT, MARLIS G. *Les origines de la seconde guerre mondiale*. Documents histoire, no. 8. Paris: Presses Universitaires de France. 1974. Pp. 136.
- SUPER, R. H. (ed.). *Matthew Arnold: Philistinism in England and America*. Vol. 10. Ann Arbor: University of Michigan Press. 1974. Pp. viii, 614. \$15.00.
- VANSITTART, PETER. *Worlds and Underworlds: Anglo-European History through the Centuries*. London: Peter Owen; distrib. by Humanities Press. Atlantic Highlands, N.J. 1975. Pp. 316. \$14.25.
- WALLBANK, T. WALTER, et al. *Civilization: Past and Present*. 4th ed.; Glenview, Ill.: Scott, Foresman. 1975. Pp. 919, 14 maps. \$14.95.
- WILSON, THEODORE A. (ed.). *WW2: Readings on Critical Issues*. New York: Charles Scribner's Sons. 1975. Pp. 515. Cloth \$12.50, paper \$3.95.
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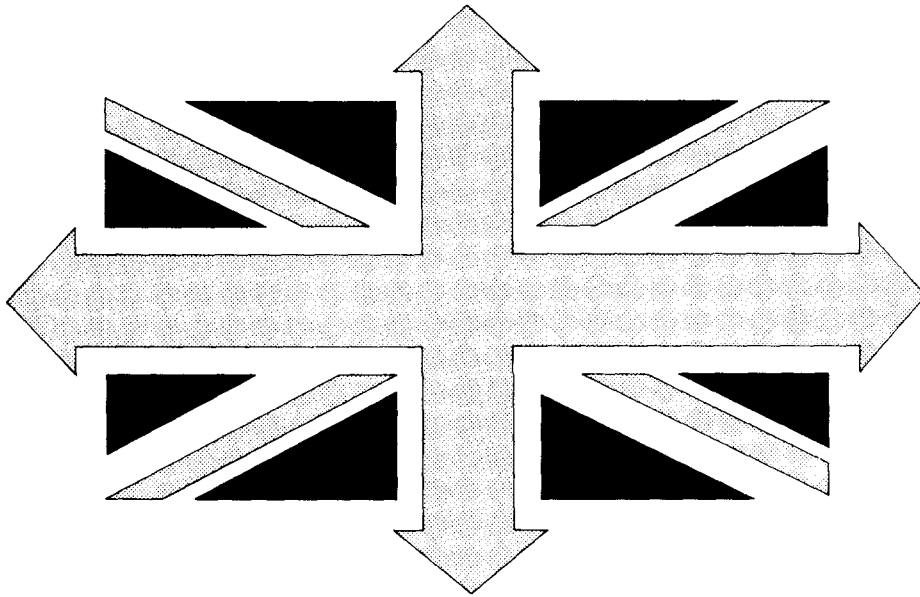
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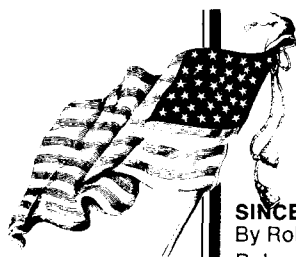
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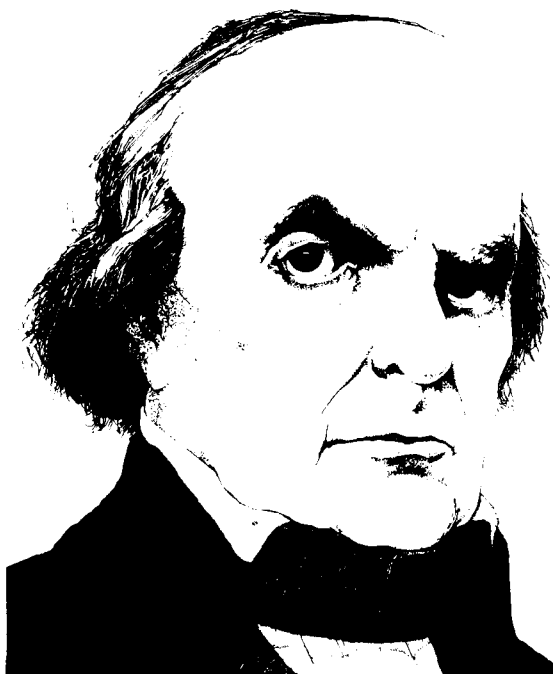
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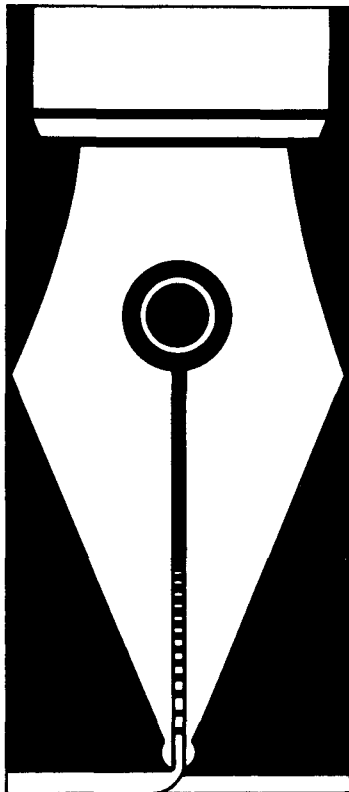
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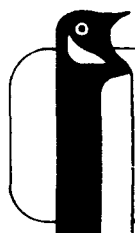
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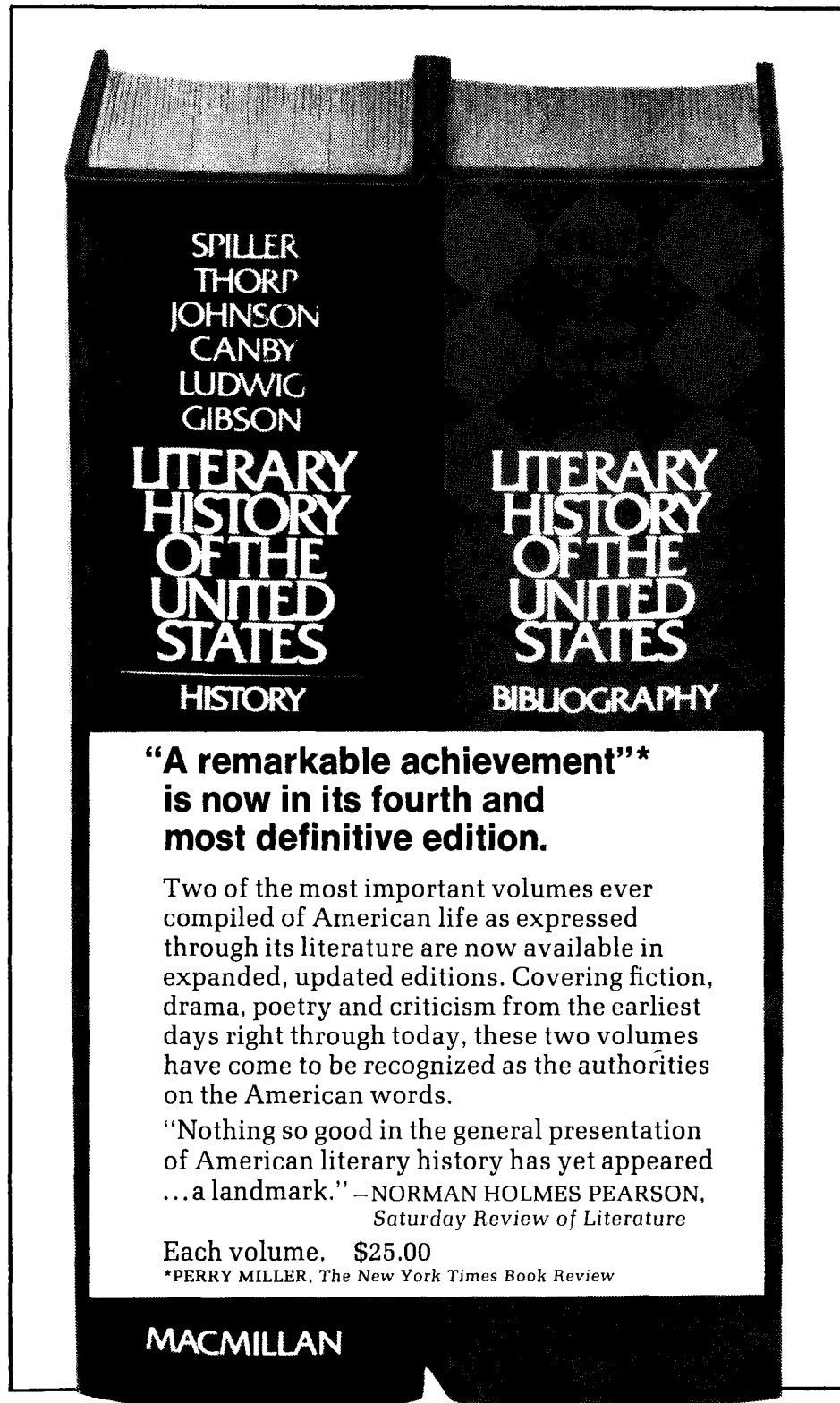
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